THE
AMERICAN NATURALIST

VOL. XLIV *March, 1910* No. 519

THE IMPERFECTION OF DOMINANCE AND SOME OF ITS CONSEQUENCES[1]

DR. C. B. DAVENPORT

STATION FOR EXPERIMENTAL EVOLUTION, COLD SPRING HARBOR, N. Y.

IT has long been recognized that in Mendelian heredity dominance is frequently imperfect. Mendel himself appreciated this fact, for he defines dominant characters as those "which are transmitted entire, or almost unchanged, in the hybridization" and points out that the hybrids between purple-red flowered and white flowered peas have lighter flowers than the darker parent. Indeed, in his letters to Nägeli he speaks of the *hybrid-form* of a character, recognizing that the hybrid character is not always that of one parent. Practically all hybridizers of the last nine years, and especially Correns and Bateson, have observed and laid emphasis on this imperfection of dominance. I can only add my testimony to theirs. It will be well to illustrate the general principle by some concrete examples. Bateson, and later Baur, found in hybrids between the prickly and non-prickly capsuled Datura that the prickles were much reduced in size. Correns found the hybrids between green- and yellow-leaved Mirabilis to have light green leaves; and in the same plant, variegation, though recessive, may be detected in the heterozygotes. Further, Mirabilis typica, 90 cm.

[1] A paper read before the American Society of Naturalists, December 29, 1909.

tall, crossed with *nana,* 40 cm. tall, produces offspring of 70 to 85 cm., thus failing to attain the dominant stature. Shull finds, in Shepard's purse, the heterozygote between elongated and non-elongated primary lobe to be imperfectly elongated, and the same investigator has devised a neat chemical experiment illustrating imperfect dominance. Morgan states that in hybrids between gray and albino rats, gray fails on the belly. In Lang's snails uniform shell color dominated over bands, but the hybrid shows pale bands. In poultry, extra toe dominates imperfectly over normal toe; complete inhibition of shank feathering dominates imperfectly over non-inhibition, so that some feathers appear; median comb dominates over absence so imperfectly that it is much reduced in the hybrids; dominant whites crossed with black give speckled females. Thus, the discoverer of dominance and his followers have generally recognized and laid emphasis on its imperfection; and were it not for certain careless exponents of Mendelism and a human tendency to substitute a skeleton formula for the living truth this introduction might have been spared.

Of imperfection of dominance there are all degrees, as Correns pointed out in 1905. The extreme case is complete failure to dominate; it has been observed many times. Thus Lock found that while black maize dominates over white, whites are in excess of expectation in subsequent generations and some of them prove to be heterozygous. Lang found hybrids between red and yellow snails to give exceptionally the recessive yellow. In poultry, median comb crossed with no median will give no median in 5 to 10 per cent. of the offspring. Extra toed crossed with normal poultry give over 20 per cent. of the recessive normal-toed type. The inhibitor of the narial flap when crossed with narrow nostril fails to activate in fully half of the offspring. Finally, cases are not unknown, as in certain rumpless fowl, when the dominant inhibitor has failed to act in the heterozygote completely. Several such cases have in recent years come to

light and it has been said that they show a *reversal* of dominance, *i. e.*, the normally recessive character has become dominant. Such an expression runs counter to the modern interpretation of dominance, which is that dominance depends on the presence of a determiner, recessiveness on its absence. Clearly a given character can hardly be now due to a determiner and again to its absence. The more reasonable hypothesis is that a determiner, though present, may fail to complete its ontogeny.

An insight into the cause of this failure is given by modern studies in cytology and breeding. These indicate that, ordinarily, in pure races, a well-developed, pure-bred character has a double determiner as its embryological anlage, while in the heterozygote the determiner is simplex. Now in just these cases when the anlage is simplex the character develops imperfectly; and it is not difficult to understand how, under certain circumstances, the simplex determiner might be insufficient for the development of the organ. Such would result in complete failure to dominate, not a reversal of dominance.

Further proof that dominance is not reversed, but only weakened, is derived from the facts of retardation in the ontogeny of some heterozygotes. For example, the white of the Leghorn is dominant over black, but, in the females at least, the dominance is so imperfect that the plumage of the hybrid shows many black spots or is spangled or it may be of a uniform blue; but in later molts it becomes pure or nearly pure white. Similarly, Lang found that the hybrid snails between the red and yellow forms sometimes showed at first the recessive yellow, but later gained the dominant red. The ontogeny of the heterozygote characteristic thus appears retarded and frequently fails altogether to reach its final goal.

This failure of the simplex determiner of the heterozygote to develop the corresponding characteristic is frequently found in one sex and not in the other. For example, the simplex determiner for horn production in sheep suffices to induce horns in the males but not in the

females. In the same way the simplex determiner induces color blindness in males but nothing less than the duplex determiner suffices to stimulate color blindness in females. One may say that, under the influence of testicular internal secretions the simplex determiner suffices for development, but not otherwise. No doubt the same is true for many other cases of "sex-limited heredity."

Imperfection of dominance goes even further. It is evinced not only in the heterozygote but even in pure-bred stock. This is the same as saying that, in breeding pure stock, a character may fail to develop even when a double determiner is present. Thus, in pure bred strains of Houdans or Dorkings having 5 toes on each foot, from 3 to 4 per cent. of the offspring fail to develop the extra toe on either foot. In certain races, at least, such as my Tosa fowl, two parents, both with complete inhibition of foot feathering, may have offspring in which this inhibitor fails to activate, so that their feet are slightly feathered. These cases of failure of a duplex determiner to work itself out in ontogeny are of the nature of sports—at least many sports are of the nature of defects arising in a pure-bred strain. In poultry one frequently gets such defects as failure of the neural tube to close (spina bifida), failure of the lower jaw to develop, absence of appendages and so on. In man, imbecility seems sometimes to occur as a sport; and it is doubtless a typical defect, transmitted as a recessive quality. It is probable that the same is true of hairlessness in mammals and man (Waldeyer, 1884, page 114, and Davenport, *Science,* November 20, 1908, p. 729). Since characteristics may occasionally fail of development in homozygotes their regular imperfection in heterozygotes is easier to understand.

Important consequences flow from the complete failure of a heterozygous characteristic. The first is, as pointed out by Shull, the difficulty of determining in the first hybrid generation what is recessive—since impotent dominance and recessiveness yield the same result. Indeed, one might be tempted to conclude with Shull that

when a character regularly fails to dominate it will never be practicable to distinguish the dominant and recessive conditions. But the situation is not so desperate as that—one clear difference between dominance and recessiveness remains, namely, while the dominant character, even when of duplex origin, sometimes develops fully, sometimes imperfectly, sometimes fails altogether; the homozygous recessive condition yields, on the other hand, offspring with entire absence of the quality. Since we can not know our homozygous recessives in advance the practical method of determining them is as follows: mate similars; some will yield families that show a great range of variation in the given characteristic from high development to complete absence, such are dominants or heterozygotes; others will yield families that show a limited range of conditions and will all be like the parents—such are homozygous recessives. Their progeny are uniform because absence of a character can not show a variability in ontogeny.

The foregoing principle may be illustrated by an example. In polydactyl strains no family (of over 2 individuals) from two syndactyl parents fails to produce some normal children, but several families from two non-syndactyl parents produce in a total of 119 offspring no syndactyl individual. The invariable families are certainly the product of two recessive parents.

The fact of imperfect dominance bears upon the controversy of alternative *versus* blending inheritance. When a booted fowl is mated to a clean-shanked one the offspring show grades of boot ranging from 0 to 9;—10 being the heaviest grade recognized. For example, when a Cochin is crossed with a White Leghorn the average grade of booting is about 4 on a scale of ten. Booting would seem to be a typical blending character. Yet the evidence of segregation is excellent. For, two extracted recessives (full-booted) beget no clean shanked offspring. $DR \times RR$ gives 50 per cent. of the offspring of grade 5 or over, $DR \times DR$ yields 25 per cent. of grade 5 or over,

while $DR \times DD$ gives none in these grades. A similar result is found in the apparently blending character of nostril height. May not segregation occur in the cases of apparent blending? Does not true blending occur only in complex characters such as stature and skeletal weight? Of such character complexes the component units may still segregate.

Finally, the fact of imperfect dominance leads to an explanation of many puzzling cases of apparent failure of inheritance. Some years ago I bought two tailless cocks *A* and *B*, of which *B* was said to be the son of *A*. They were certainly very similar in appearance. I mated *A* to some tailed hens and their offspring were tailed. The next year I mated these hybrids with each other and the females with their father. Were taillessness recessive, as I suspected, one-fourth of the progeny of the first mating and half of the progeny of the second should have been tailless. Actually there was produced not one tailless bird. One would apparently have been justified in concluding that taillessness is not an inheritable condition. But when the second tailless cock was mated with the tailless hybrids approximately half of the offspring were tailless; and such tailless offspring bred *inter se* have in turn produced a large proportion of tailless progeny. This whole case at first seemed inexplicable to me, as it did to Professor Bateson, to whom I related it, but it receives a satisfactory interpretation on the theory of imperfect dominance. For rumplessness, or rather an inhibitor of tail growth, is dominant over its absence. But with cock *A* this inhibitor is so impotent that in the heterozygote, at least, it does not make itself felt and even in the second hybrid generation the duplex determiner fails to activate fully. I say *fully*, for there was a trace of activity. At least fifteen per cent. of the offspring were recorded as having a small uropygium and in many of the adults the back appeared shortened and bent and the tail drooped instead of standing erect. Despite these evidences of the activity of the inhibitor, the

striking fact is that its activity is so feeble that it cannot prevent the development of the tail. In the case of the second cock, however, the inhibitor is stronger and behaves more nearly according to Mendelian expectation. Now, if a character can be so feeble as to fail completely in development in the heterozygote and even in the homozygote it will give the impression of non-inheritability; and I have little doubt that many cases in which there is apparently no or only a slight inheritance are due to a weak determiner. I could cite a considerable number of cases of this sort in my experience, but I refer for an account of them to a book that is being published for me by the Carnegie Institution of Washington. Still one other lesson may be drawn and that is the apparent variability in what I call the *potency* of determiners. The evidence for this potency seems to me to be strong, as it certainly is important for an interpretation of all non-Mendelian cases of heredity. By the aid of the facts of imperfection in dominance and the hypothesis of varying potency of determiners the territory to which the principle of the segregation of determiners is applicable becomes greatly extended.

EXPERIMENTAL EVIDENCE ON THE EFFECTIVENESS OF SELECTION[1]

PROFESSOR H. S. JENNINGS

JOHNS HOPKINS UNIVERSITY

IN studying the problems of evolution in the common infusorian *Paramecium,* I found that by methodical and progressive selection striking results can be reached.

From a wild culture it is possible by progressively selecting in two opposite directions to obtain finally two lots, one of which is many times as large as the other, and the differences between the two are permanent and hereditary. By properly regulated selection a great variety of permanently differentiated lots are obtainable.

Throughout this work Galton's law of regression was found to hold; that is, the progeny of extreme parents inherited the peculiarities of their parents, but in a less marked degree. Furthermore, the results were such as to lend themselves readily to interpretation as exemplifying Galton's law of ancestral heredity.

Thus the effectiveness of selection was clearly demonstrated. But just what sort of effectiveness does the theory that selection is the dynamic factor in evolution demand? It demands that selection shall so act that it might finally produce progress from *Amœba* up to man. It must produce, from a given condition, something that did not before exist in that given condition.

Has selection so acted in this case? To answer this question, we must evidently know precisely what exists in the condition with which we start. We therefore next work with the progeny of a single individual—forming a "pure line," the characteristics of which we thoroughly know.

Now we try the effects of selection on this pure line.

[1] A paper read before the American Society of Naturalists, December 29, 1909.

Not the faintest trace of effect is produced, even by long-continued methodical selection for hundreds of generations. The race or line is absolutely permanent, so far as the appearance of any hereditary differences are concerned. The individuals of the line do indeed differ greatly among themselves, but these differences are not inherited; they furnish absolutely no foothold for selection.

Examination showed that *Paramecium* consists of many such races, differing among themselves slightly, but each race as unyielding as iron. And the extreme races found in the wild culture are precisely the extremes obtainable by long-continued selection.

The effects of selection have then consisted simply and solely in isolating races that already existed. It had produced nothing new; there had been no progress that would form a step, however slight, in the journey from *Amœba* to man.

When I had reached this point I looked about and found that others had been having similar experiences. The investigator who discovers these things for himself finds perhaps that

> Es ist eine alte Geschichte
> Doch bleibt sie immer neu.

And the second line is as true as the first, for to one who has put months and years on such attempts to accomplish results by methodical selection, its utter powerlessness comes with new and surprising force. Johannsen in working with beans and barley, Hanel with *Hydra,* had found many pure lines existing in nature, but as in my own case, each pure line was absolutely unyielding.

But we know that others *had* found selection effective; a whole series of cases comes at once to our lips. Galton in studying peas and men; Fritz Müller with maize; de Vries with maize and with buttercups, MacCurdy and Castle with guinea pigs and rats—all these had reported definite progress as a result of methodical selection.

Why this difference? Is there one law for the Jews, another for the Gentiles?

Looking into the matter with care, we find that the results with our own material are, after all, like those of the investigators mentioned if we treat it in the same way. None of these workers first isolated their pure races. If we begin with a mixture we can, in beans, in barley, in *Paramecium,* in *Hydra,* by a methodical process of slow selection make gradual progress in a certain direction. But our selection is only a process of purification, and when we finally get a pure race, selection is utterly powerless to go farther. We should have been completely in the dark as to the real effect of selection if we had not carried through rigidly the "pure line" idea.

Is it possible then that we have in this pure line idea an instrument of the greatest importance for analysis? Is it perhaps the key which every one must have in order to understand the results of selection? May it be indeed one of those fundamental ideas which, like the idea of mutation, is fitted to clear and crystallize a confused and turbid mixture? Is it possibly of sufficient importance to deserve agitating a little before the American Society of Naturalists?

Let us put these questions to the practical test; let us apply the idea as an instrument for the dissection of the classic cases which seem to demonstrate the efficacy of selection in producing change of type.

Johannsen in his recent book has used the pure line concept as an instrument for analysis of the entire field of variation, heredity and evolution, and to him is due the credit of first perceiving the importance of this concept, when sharply defined, as such an instrument for research and presentation. The work of Johannsen, I believe, will remain one of the landmarks of progress in this field. But my own analysis has been independent of Johannsen's and diverse from it, developing inevitably from what I have myself seen, so that I may venture still to present some of its results.

But how can we apply the pure line idea to organisms whose lines are *not* pure; organisms that interbreed freely; organisms in which the characters of a given line split off, separate, and become exchanged for those of other lines, in the way characteristic of Mendelian inheritance?

The pure line idea here becomes a little elusive, a little abstract. But possibly it is still helpful as an instrument of analysis; let us try it. In order not to emphasize purity where impurity is the rule, let us substitute Johannsen's term *genotype* for "pure line"—defining the genotype as a set of individuals which, so long as they are interbred, produce progeny that are characteristically uniform in their hereditary features, not systematically splitting into diverse groups.

Now, how can we determine whether the genotype concept, with its consequences for the effects of selection, applies to organisms with biparental inheritance? Reflection shows that if it does, certain general propositions are true: if these propositions are found to hold, the genotypic explanation of the effects of selection is confirmed.

1. The first proposition is this: Organisms in which selection has shown itself effective are composed of many genotypes; of many races that are diverse in their hereditary characters. This we know to be true.

2. Second, from such a mixture of genotypes it is possible to isolate by selection any of the things that are present—perhaps in a great number of different combinations.

3. But from such a mixture it is *not* possible to get by methodical selection anything not present (save when rare mutations have occurred).

4. Therefore it is not possible to get by methodical selection anything lying outside the extremes of the genotypic characters already existing.

This is perhaps practically our most important proposition. For in order that selection shall produce pro-

gression from *Amœba* to man, it is evidently necessary that it should give us characters lying beyond the extremes of what already exists.

5. Our fifth proposition is that in the case of genotypes that cross-breed readily, we may get an indefinite number of combinations of all that lies between the extremes of the existing genotypes—the variety of combinations realized depending on the rules of inheritance.

Now, if we test by these propositions the classic cases of effective selection, what is the result?

Galton's work with peas and with men yields at once to the analysis, giving precisely the results which the genotypic idea requires. A by-product of the analysis is the practical evaporation of the laws of regression and of ancestral inheritance so far as their supposed physiological significance is concerned;[2] they are found to be the product mainly of a lack of distinction between two absolutely diverse things—between non-heritable fluctuations on the one hand, and permanent genotypic differentiations on the other.

The experiments of Müller and de Vries on maize yield with equal readiness. In these cases the male parents are unknown; the freest sort of crossing was occurring, and what selection did was pick out the progeny of extreme male genotypes, till the result approached the limit of the most extreme existing genotype under the cultural conditions.

MacCurdy and Castle's experiments in changing by selection the color-patterns of rats and guinea-pigs dealt

[2] This significance was supposed to lie in showing that the characteristics of the progeny depend upon the characteristics of the ancestors for many generations back, in ways that are definable. "Mr. Galton's view of the effect of regression follows inevitably from the general theory of chance, if we regard the character of an individual as a phenomenon due to a series of complex groups of causes, *among which are the characters of each ancestor.*" (W. F. R. Weldon, *Biometrika*, I, p. 370.)

The law of ancestral inheritance does not hold in pure lines, even in a statistical sense, as has repeatedly been shown. The progress of a long series of extreme ancestors does not differ from those of a series of average ancestors.

with races of complicated descent; they plunge us at once into all the difficulties due to interweaving, blending and transfer of characters from one genotype to another. But if we stick closely to the general propositions already stated, we shall have a guide. MacCurdy and Castle got by selection all sorts of conditions lying between the extremes with which they started. But did they get anything lying outside these extremes, as would be required in order to show that we can by selection make evolutionary progress? As I read their results, they did *not*. Their experiments are most important for many problems of variation and inheritance, but they do not give us evidence that methodical selection can produce anything beyond combinations of what already exists; hence they do not help us in getting from *Amœba* to man.

The work of the German breeders who have for years practised methodical selection for improvement of agricultural races clears up at once under the genotype idea, as the analyses of Fruwirth, v. Rümker and others show us. Continued methodical selection is often necessary, but what it does is to purify a contaminated race—a process which, owing to the laws of inheritance, may require several generations.

I have spoken only of those experiments which seem at first view to show the efficacy of selection; brevity requires me to pass without mention over investigations which, while not carried on with pure lines, support and reinforce the conclusions drawn from such work. Such for example are the fundamental experiments of Tower, the recent work of Pearl, of Shull, and many of the experiments of de Vries.

Thus far the dissecting knife of the pure line idea succeeds admirably in letting the light into the obscure workings of selection. Any one who uses it with precision will find what an important advance its exact formulation by Johannsen marks over even such an analysis as that given by de Vries. In the work of de Vries, as in that of many recent authors, the selection

idea is appraised at essentially its true value, but much in its action is left obscure. The reader is surprised at the accounts of experiments in which selection does accomplish marked results, though according to the general theory, it should not; one is left puzzled in judgment as to what we may expect from it. With the sharply formulated pure line concept as a guide, most of this obscurity disappears.

And then, to keep us from resting on our oars; to give us humility and spur us to further work—we come to the one case in which the pure line idea fails to bring clearness. This is de Vries's experiment with buttercups. Here, after selection the extreme was moved far beyond that before selection. Before selection the extreme number of petals was eleven; after selection it was thirty-one. Before selection no single individual had an average number of petals above six; after selection the average of all was above nine, and some had an average of thirteen! It is true that there are "mitigating circumstances" here; the work was not done with pure lines, and the variations dealt with are not of the ordinary fluctuating sort (as de Vries points out); change in cultural conditions doubtless played also a large part. Possibly repetition with thorough analytical experimentation will show that something besides selection has brought about the great changes. But at present the case stands sharply against the generalizations from the pure line work. It is the only such case that I have found.

To sum up, one finds not only that his own results and those of many other modern workers give the pure line interpretation, but also that all other cases that had seemed to point the other way yield readily to the genotypic analysis—save one. If we ride rough shod over this case, as not yet sufficiently studied, then we may draw a tentative conclusion as follows:

The pure line or genotype idea is the one to see clearly and grasp firmly in experimental investigations on selection. Many even of the modern experiments remain

obscure in their significance simply because the workers have not grasped this concept, have not shown the relation of their results to it. Further, in presenting one's own work, or in interpreting the accounts of others, the genotype concept is the instrument of precision to take in hand. The results of the analysis made by its aid indicate that most or all of the experiments in methodical selection have consisted in shifting about, isolating and recombining preexisting, permanent hereditary differentiations, giving results that were interpreted as revealing the law of actually progressive evolution, though in reality they had no relation to such a law.

To our conclusion as to the analytical value of the pure line idea we may expect strenuous opposition on the part of that last small remnant (if there be such a remnant) of the biometrical school that still submits to the dictation of Pearson[3]—for by one of those sardonic paradoxes through which nature revenges herself, the men who from outside have lectured biology on the necessity of becoming exact are the strongest opponents of exact experimental and biological analysis—seeming to feel that mathematical treatment renders other kinds of exactness undesirable.[4] Those who find the genotype idea useful may then prepare themselves for one of those justly famous bludgeonings from the dictator of the whilom orthodox biometrical school; this is the last honorable mark of distinction which stamps the investigator as a thorough and exact analyst of things biological.[5]

[3] Note how quickly the biometricians that devote themselves to careful biological investigations fall away from the Pearsonian faith. Darbishire, Davenport, Tower, Shull, Johannsen, Pearl; are there any biologists of achievement that still hold with Pearson?

[4] Pearson in 1901 informs us that evolution is a field "where no tabulation of individual instances can possibly lead to definite conclusions" (*Biometrika*, I, p. 344). This was the year of the appearance of de Vries's *Mutationstheorie*, and of the revival of Mendelism. Compare in definiteness and value the conclusions drawn from the work inaugurated in these two lines, based as it was precisely on the "tabulation of individual instances," with those from the biometrical work, with its careful avoidance of "individual instances."

[5] To name the men who have been subjected to Pearson's most savage

A word more on certain general questions. Can we conclude that if selection has no dynamic effect in changing existing genotypes, that therefore it need not be reckoned with in evolution? Or must we conclude that if it is to be reckoned with at all, selection has opportunity to act only on large leaps in evolution; that evolution takes place by such leaps, and not by imperceptibly small changes?

Such evidence as the pure line work gives implies neither of these things. The differences between the diverse pure lines *have* arisen in some way, if evolution occurs, and once these differences have arisen, they are open to the operation of selection as are any other differences. What the pure line work shows (agreeing in this with other lines of evidence) is that the changes on which selection may act are few and far between, instead of abundant; that they are found not oftener than in one individual in ten thousand, instead of being exhibited on comparing any two specimens; that a large share of the differences between individuals are not of significance for selection or evolution—these being precisely the differences measured as a rule by the biometrician's "coefficient of variation." Thus the work of natural selection is made infinitely more difficult and slow; but logically it is still possible.

Nor does the pure line work assist natural selection, as some have hoped from the mutation work, by making the steps in evolution greater in amount. On the contrary, the work with genotypes brings out as never before the minuteness of the hereditary differences that separate the various lines. These differences are the smallest that can possibly be detected by refined measurements taken in connection with statistical treatment. Johannsen found his genotypes of beans differing constantly merely by weights of two or three hundredths of a gram in the average weight of the seed. Genotypes

assaults is to name the men that have done most to advance our knowledge of heredity. The cases of Castle and of Bateson will occur to every zoologist.

of *Paramecium* I found to show constant hereditary differences of one two-hundredth of a millimeter in length. Hanel found the genotypes of *Hydra* to differ in the average number of tentacles merely by the fraction of a tentacle. That even smaller hereditary differences are not described is certainly due only to the impossibility of more accurate measurements; the observed differences go straight down to the limits set by the probable error of our measures. Genotypes so differing have not risen from one another by large mutations. The genotypic work lends no support to the idea that evolution occurs by large steps, for it reveals a continuous series of the minutest differences between great numbers of existing races.

All together, I think we may say that the pure line or genotype concept presents an instrument of analysis which is worthy, on the basis of what it has thus far done, of a thorough tryout for future work, and no one interested in these questions can afford to neglect it. This conclusion is quite independent of the concrete results reached; the efficacy of selection in modifying genotypes may be demonstrated to-morrow, but the demonstrators will need to show precisely the relation of their results to the pure line concept.

THE ARTIFICIAL PRODUCTION OF THE PARTHENOGENETIC AND SEXUAL PHASES OF THE LIFE CYCLE OF HYDATINA SENTA

A. FRANKLIN SHULL

COLUMBIA UNIVERSITY

THE causes that determine the transition from the parthenogenetic to the sexual mode of reproduction in the rotifer *Hydatina senta* have been the subject of a number of investigations, which have led to contradictory conclusions. Maupas was led to attribute the appearance of sexual forms to changes of temperature, Nussbaum to starvation. Punnett found that neither temperature changes nor starvation had any effect, but obtained what seemed to be distinct strains, each producing a fairly constant proportion of sexual forms, regardless of external conditions. One strain produced many sexual females (about 40 per cent.), another few sexual females (2 per cent.), and a third none at all. His conclusions regarding temperature and starvation were supported by Whitney, but the latter worker obtained no evidence of strains, finding that so-called strains of one type might break up into those of any other type within a single parthenogenetic series.

I believe I have evidence which may explain the difficulties hitherto encountered, evidence that goes far towards bringing the previous contradictory conclusions under a single point of view. I shall attempt to show in this brief presentation that the presence of substances in the water, other than food, exerts a strong influence on the inauguration of the sexual phase.

In this paper I shall speak of the male-producers as sexual females. The identity of the two has long been suspected from certain numerical relations, but more

direct observations have been wanting. I have now, however, been able to secure winter eggs and males from the same parent. Since in all cases known, a female lays parthenogenetic eggs of only one sex, there can be little doubt that sexual eggs and male eggs are identical.

To test the influence of substances in the food cultures, lines of rotifers were reared in the water of some old cultures, from which the protozoa had been removed by filtering. Various dilutions of this water were used, one fourth, one half, three fourths and undiluted, as well as pure spring water. The same kind and quantity of food, a flagellate, *Polytoma uvella,* was added to each, and was found to live readily in all concentrations of the filtrate. The results of eight or more generations bred simultaneously under these conditions are shown in Table I.

TABLE I

	Spring Water.		Old Culture Filtrate.							
			One fourth.		One half.		Three fourths.		Undiluted.	
	Sex. ♀	Parth. ♀	Sex. ♀	Parth. ♀	Sex. ♀	Parth. ♀	Sex. ♀	Parth. ♀	Sex. ♀	Parth. ♀
	26	177	25	407	15	350	8	362	0	337
Per cent. Sex. ♀	12.8		5.7		4.1		2.1		0.0	

There is a gradual decrease in the proportion of sexual females from the line bred in pure spring water to that in the undiluted filtrate. The series in the dilute filtrate were discontinued at this point, but those in spring water and in concentrated filtrate were continued. In the latter line, nineteen complete generations were obtained without a single sexual female, while in spring water the sexual forms recurred at intervals, though not frequently, to the end of the experiment.

The experiment was repeated by removing to spring water a female of the seventh generation of the series bred, in the preceding experiment, in concentrated filtrate. The results of this change are shown in Table II. Six generations were reared in each line.

TABLE II

SPRING WATER.			OLD CULTURE FILTRATE.		
Sex. ♀.	Parth. ♀.	Per cent. Sex. ♀.	Sex. ♀.	Parth. ♀.	Per cent. Sex. ♀.
33	216	13.2	0	174	0.0

From the third generation of the series in spring water in Table II, a female was returned to the concentrated filtrate, and her progeny reared for three generations. A control was continued in spring water. The results are those in Table III.

TABLE III

SPRING WATER.			OLD CULTURE FILTRATE.		
Sex. ♀.	Parth. ♀.	Per cent. Sex. ♀.	Sex. ♀.	Parth. ♀.	Per cent. Sex. ♀.
7	110	5.9	0	107	0.0

The experiment was repeated four times, with sisters in each case, derived from lines more or less distantly related to those in the preceding experiments. These are recorded in Table IV.

TABLE IV

No. of Exp.	SPRING WATER.			OLD CULTURE FILTRATE.		
	Sex. ♀.	Parth. ♀.	Per cent. Sex. ♀.	Sex. ♀.	Parth. ♀.	Per cent. Sex. ♀.
1	44	163	21.2	0	141	0.0
2	49	64	43.3	0	128	0.0
3	7	106	6.1	0	103	0.0
4	5	76	6.1	0	85	0.0

In every case, the evidence points to the same conclusion, namely, that the filtrate from the old cultures reduces the number of sexual females. The losses by death are too few to account for the results obtained, and there is evidence to show that what deaths there are are not selective.

It is interesting to note the bearing of the above evi-

dence on the question of starvation. It is not practicable to diminish the quantity of food placed in the dishes, without at the same time diminishing the quantity of other substances introduced. Several experiments were made to test the apparent effect of starvation. When the young were isolated, they were given only as much food as was judged necessary to support life and enable them to produce a moderate family. A control line, which was fed abundantly from the same cultures, was maintained in each case. One experiment, Table V, included 55 generations and covered a period of three months. In this time, the well-fed control varied so greatly in the proportion of sexual to parthenogenetic females produced, that it is important, not merely to note the totals, but to divide the experiment into periods, and compare the results in each.

TABLE V

Limiting Dates.	WELL-FED.			STARVED.		
	Sex. ♀	Parth. ♀	Per cent. Sex. ♀	Sex. ♀	Parth. ♀	Per cent. Sex. ♀
July 23–July 27	127	73	63.5	72	47	60.5
July 28–Aug. 25	50	754	0.6	125	265	32.0
Aug. 26–Sept. 5	129	153	45.7	55	80	40.7
Sept. 6–Sept. 20	35	203	14.7	34	100	25.3
Sept. 21–Oct. 12	187	261	41.7	93	157	35.2
Oct. 13–Oct. 25	25	208	10.7	87	161	35.0
Total	553	1,652	25.0	466	810	36.5

While the experiment as a whole shows a decidedly higher percentage of sexual forms in the starved families, there are parts of it that show a slightly lower proportion in the starved line. The variation in the percentage of sexual females from one period to the next in the starved line is of the same sign as in the corresponding periods of the well-fed line, but is smaller in every case. While the major fluctuations of the starved line occur simultaneously with those of the well-fed line, they are less in degree; the extremes of the starved series are always well within those of the well-fed. I can find only one

factor which operated on both lines in the same way, but to less degree in one line than the other, namely, the food culture.

Of especial interest are the second and sixth periods Table V. In these, not only the *percentage*, but also the *absolute number*, of sexual females is higher among the starved individuals. This shows that the percentage is not raised by the mere elimination of some of the parthenogenetic females.

The experiment with starvation was repeated three times, each repetition including eight to sixteen generations. Each time there was a considerably higher proportion of sexual females in the starved families.

While it is conceivable that several factors may be at work producing these different proportions of sexual to parthenogenetic forms in the well-fed and starved families, the experiments with the filtrate from old food cultures show that the different quantity of dissolved substances incidentally given with the food is sufficient to explain the results. If this is the correct interpretation, the larger proportion of sexual forms in the starved families is not due to lack of food, but to the absence of chemicals which, in the well-fed families, prevent the appearance of the sexual forms.

THE SIGNIFICANCE OF THE COURTSHIP AND SECONDARY SEXUAL CHARACTERS OF ARANEADS[1]

PROFESSOR THOS. H. MONTGOMERY, JR.

UNIVERSITY OF PENNSYLVANIA

BUT few observers have made special studies in those phenomena in spiders of which it is proposed to treat in the present communication. The first studies, by Canestrini, were inaccessible to me. The main work on the subject is that of Professor and Mrs. Peckham (1889, 1890). They have described in great detail the courtship of a considerable number of Attidæ, and have given excellent illustrations of the attitudes of the males. Then after an analysis of the phenomena in question they have criticized the views of Wallace (1889), and have accepted that part of the sexual selection theory of Darwin which accounts for secondary sexual differences on the basis of an esthetic discrimination by the female. The results of these studies have been generally accepted as one of the strongest confirmations of Darwin's views. In the introduction to their last memoir (1909) they have reiterated their arguments, with the addition of certain new and important observations. The next special work on this subject is my paper of 1903, in which is described in detail the courtship of certain lycosids, agelenids, dictynids, theridiids, pholcids, epeirids and thomisids. My general theoretical conclusions were quite different from those of the Peckhams: the adult male is excited simultaneously by fear of and desire for the female, and his courtship motions "are for the most part exaggerations of ordinary motions of fear and timidity. By such motions he advertises himself to the female as a male, but

[1] This paper was presented before the American Society of Naturalists, Boston, Mass., December 29, 1909.

there is no proof that he consciously seeks to arouse her eagerness by esthetic display . . . there seems to be no good reason to hold that the female is actuated in her choice by sensations of beauty." Thus my opinion was opposed to Darwin's theory. The remainder of the literature on the secondary sexual phenomena of araneads contains for the most part only casual observations, without attempts at analysis.

I. Some General Mating Relations

For a correct understanding of the points at issue it would be very desirable to have ample data on the numerical proportions of the sexes, but unfortunately little is known on this head. What is known is brought together in another paper of mine (1908*a*); and there also it was found in the case of *Lathrodectus mactans* that the "average male ratio (number of the males divided by the number of the females) is 8.19, determined from a count of 41,749 newly hatched spiderlings." In this species the adults differ most markedly, and the young emerging from a cocoon are readily separable into two groups, distinguishable not only by size relations, but also, as specially indicated by me, by differences in the form proportions of the abdomina. Those spiderlings with a flatter dorsum and more anterior pedicel were considered to be males, for this is one of the characters in which the adults differ. However, none of these spiderlings were raised to maturity, though the attempt was made to do so; no differences in the internal genital organs can be found at the time of hatching, and occasionally (though rarely) nearly intermediate forms occur between the two groups of spiderlings. There is then room for doubt whether the two groups represent males and females, respectively, and the sex-ratio is not securely established from my results; it is rather in the nature of a strong probability. Beyond this case nothing definite is known about the numerical proportions of the sexes.

It is the general rule that the males mature earlier than the females, and the former do not live longer than a year. Promiscuous mating is general, a male impregnating a number of females, and a female receiving a number of males. I have seen a female of *Geotrecha pinnata* mating with two males in close alternation, two males of the theridiid *Ceratinopsis interpres* embracing a female simultaneously, and I have described (1903) for *Theridium tepidariorum* as many as twenty-seven impregnations of one female. In the attid *Phidippus purpuratus* one male was seen by me to fertilize several females. Monogamy is exceptional, and would appear to occur in cases where the male seizes immature females by force, and where the male lives in a mating nest with a female. Many attids make such nests; in the case of *Phidippus purpuratus* I have frequently found pairs in such nests in the wild state but never found them to make such nests in captivity, where there is on the contrary promiscuous mating. McCook (1890) has brought together some of the literature on mating nests. In certain cases an adult male seeks out and sequestrates an immature female, mating with her when she matures. The Peckhams (1889) found the only attid under their observation, "in which we saw males take possession of young females and keep guard over them until they became mature," to be *Philæus militaris.* A pair of *Drassus neglectus* was caught by me in a mating nest on June 16, last, and placed in a cage, where he built another nest around her; on June 23 he mated with her when she had just finished moulting and was still quite soft; in another case a male and an immature female were found in the same nest. A mature male of another drassid, *Prosthesima atra,* was repeatedly observed by me to hold an immature female, and on another occasion to grasp two such at once. In *Theridium tepidariorum* adult males wait upon the snares of immature females, and this has been seen by McCook (1890) and me in various epeirids. In *Lycosa ocreata* I found that

males pay no attention to immature females, while they do in *L. scutulata.* During the past summer I have seen males courting immature females in the attid *Zygoballus beltini,* the thomisid *Xysticus nervosus,* and the drassid *Chiracanthium inclusum.*

In some species, as notably epeirids and lycosids, pregnant females are hostile to males. But I have seen such females of *Theridium tepidariorum* and *Phidippus purpuratus* receive males, and on one occasion a female of *Geotrecha pinnata* was interrupted several times during her cocooning by embraces of a male. Males appear to court any mature female, whether she be virgin or not.

II. Senses Employed in Sexual Recognition and Stimulation

Here we have to consider briefly the rôles of hearing, touch, sight and smell.

There is no good evidence that spiders possess hearing, while the arguments of Wagner (1888) and especially the direct observations of Pritchett (1905, done under my direction) speak almost conclusively against such possession. Some spiders have stridulating organs, and in certain species these are limited to the male (*Theridium,* according to Westring), but no experiments have been made to determine whether the spiders react to the sounds produced thereby. I have recently found in the drassid genus *Geotrecha,* where both sexes possess a stridulating apparatus, the spiders do not in any way indicate any perception of sound but perceive each other solely by touch. It is then fairly firmly established that the sexes do not recognize each other by hearing.

Touch is the dominant sense, and would appear to be the especial function of the jointed spines. In nocturnal species as well as in all snare-weavers it appears to be the only sense of sex-recognition.

Smell is possessed by spiders, but what organs sub-

serve it is not determined, beyond that it seems to be distributed over a considerable area of the body. The most careful study of it has been made by Pritchett (1905), while Petrunkevitch (1907) found it to be less keen than touch. It might be this sense that guides wandering males in their search for females. But I am inclined to believe it does not, for of pairs I have watched attentively in cages the male always appeared to find the female by either touch or sight, while if he is near her, but without seeing or touching her, he seems unconscious of her proximity. Further, when in the wild state a male approaches a female upon her snare, there seems to be no evidence that he constantly approaches her in the direction of the wind. Thus it is very doubtful whether scent has any part in sex-recognition in spiders, while it is the most usual mode of sex-recognition in insects.

Sight undoubtedly plays a considerable part in sex-recognition in certain diurnal species, determined for the attids by the Peckhams, and for the lycosids by me. These spiders, however, perceive readily only moving objects. But, as we shall see, it is not used for this purpose in the other spider families, not even in the diurnal thomisids. Thus I can not agree with Petrunkevitch's conclusion (1907) that "the sense of sight is beyond any doubt the only sense that guides hunting spiders on their hunting excursions and in finding the females during the mating period," for I have frequently noticed males of even diurnal attids and lycosids first recognizing the female by touch. And in the case of *Pardosa nigripalpis,* a species that always courts by light, it was remarked by me (1903): "In a double cage with a transparent glass partition, a male in one compartment and a female in the other, I have not seen a male court a female, though he certainly sees her through the partition; probably, then, it is touch of a female that impels him to courting activity." Numerous pairs were kept by me in such cages to test this point.

The senses of sex-recognition are, accordingly, touch in the first place and sight in the second, the latter important in only a few families.

III. Details of the Courtship Phenomena

It will be convenient for our present purposes, for it is based upon habitudinal differences, to divide the species on which observations have been made into the two categories of "snarers" and "hunters." The literature on the subject has been gleaned as thoroughly as possible; and my new observations of the past summer have been presented more fully than the others simply because they are here given for the first time.

1. *Snarers*

In *Dictyna volupis* Keys. mature males and females live together upon the web, which they appear to fabricate in common; the male is somewhat larger. In two cases the approach of the sexes was seen by me (1903): in one the male approached and seized the female; in the other case a male came face to face with a female, "then, each of them tapping upon the web with the first two pairs of legs, they moved backward and forward slowly. This lasted only two minutes, when they both became quiet half an inch apart; it was repeated again for a short period." These are the only observations on any cribellate form, and they do not indicate clearly whether rape or courtship by the male is the usual process.

For the Agelenids Menge (1843) described the approach of the male in *Agelena labyrinthica* as follows: The male climbs into the web of the female, taps on it with his palpi, but must sometimes wait for an hour before the female allows him to approach. "When she is complacent she places her legs close to her side, and the male embraces her with his legs and carries her into the funnel." In *A. nævia* Walck., I saw the approach

of the male in two cases; in both there was nothing "to indicate a courtship; there was simply a cautious approach of the male," he touched her with his first legs, "and after he had found no sign of hostility on the part of the female he quickly seized her, and she was absolutely submissive in his grasp." In *Tegenaria durhami* Scop., in several cases, the male was seen by me to slowly approach the female upon her web, he tapping the latter with his fore legs as he advanced, the female occasionally also tapping in response, after which he would make a sudden rush at her. In these agelenids there are no courtship motions beyond tapping on the web, to which the female may respond, in like manner; the male seems to find her by touch, and after a cautious approach to seize her forcibly.

In *Pholcus phalangioides* Fuessl. I found (1903) no courtship; the male approaches the female very cautiously upon her web, and touches her gently with his legs; he always approaches her from beneath, and immediately drops from the web if she move at his touch. He finds her position by carefully pulling the lines of the snare.

In the theridiids a number of observations have been described. Menge (1843) found in *Linyphia triangularis* that the approaching male shakes the snare rapidly, to which the female responds by a similar motion. In *Theridium tepidariorum* C. Koch I have seen (1903) the approach of the sexes many times; when a male is placed upon a web of the female, she immediately signals to him by repeated jerks upon the lines of the web, sometimes moving towards him. This signalling is a sign of eagerness on her part, and "she makes it at no other time than when she is eager and notices the approach of a male of her own species. . . . The whole attitude of the male is that of combined timidity and great eagerness. . . . He tests the eagerness of the female, and finds her position upon the web, by grasping with the claws of his first pair of feet the web lines that she is

shaking by her signalling, and by drawing these web lines taut he feels her movements all the more distinctly; he approaches gradually nearer her, guided by her signalling, and finally makes a short rush toward her." The female often seems insatiable, even leaving food at times to approach the male, and the courtship is largely on her part, by signalling.

Similar signalling by the female was noticed by me in *Teutana triangulosa* Wolck. During the past summer I watched the process in *Theridium frondeum* Htz., where the female signals, and the male responds in the same way; the female had her rear turned towards him so that she could not possibly see him; in one case a female signalled to another female. In the case of the tiny *Ceratinopsis interpres* Emert. I placed two males and a female together in a vial, where they spun a maze of lines in a short time, but I observed quite a different approach of the males. The female does not appear to signal, but the male makes a quick rush at her, and taps her rapidly with his legs until she becomes submissive with contracted legs. A male acts towards her as he would to another male, seemingly aggressive, until she becomes immobile; they find one another by touch communicated along the web lines, not by sight.

In the Epeirids a number of genera have been studied. In *Pachygnatha listeri* Menge (1866) saw the male seize with his chelicera those of the female. In *Argiope* (Peckham 1889, McCook 1890, Emerton 1883) the male courts the female by pulling the radii of her snare, and she responds in the same way. "If matters be favorable, the male draws nearer, usually by short approaches, renewing the signals at the bolting places. Sometimes this preliminary stay is brief; sometimes it is greatly prolonged" (McCook). In the case of *Acrosoma gracile* Walck. I dropped (1903) a male upon the web of a female, and as soon as he touched her (within five minutes) he copulated; no special courtship was seen. In the genus *Epeira* the approaches of the male are well known from

the observations of Walckenaer (1837), Menge (1843, 1866), Termeyer (1866), Lendl (1886), McCook (1890) and myself (1903, 1908*b*). The male approaches the female cautiously upon her web, locating her and testing her eagerness by pulls upon a radius, she responding gently to his signals when she is eager, otherwise she makes a sudden rush at him in which case he swings free from her web on a particular line of his own.

When we sum up what is known of snare-weavers, it appears that in them sex-recognition is always by touch, by tapping or pulling of the web. In a few cases (*Tegenaria, Ceratinopsis* and perhaps *Dictyna*) the male attempts to capture the female by storm; but more generally he approaches very cautiously and timidly, signalling to the female. When there is a courtship it is one of line signals, and sometimes (*Theridium*) the female seems the more eager and active in the courtship.

2. *Hunters*

For the drassids, most of which are nest makers and essentially nocturnal, there are the following observations. Menge (1872) placed together a male and female of *Melanophora nocturna,* whereupon the male immediately embraced the female without courtship; and he observed (1873) a similar forcible rape in *Chiracanthium oncognathum.* In *Clubiana trivialis* he found (1873) that the male makes a small saccular nest next that of the female, and that he knocks, sometimes for days, upon her nest wall before she allows him to enter. During the past summer the following observations were made by me. In *Drassus neglectus* (Keys.) an adult male appropriates an immature female, seals her in a mating nest, and mates with her just after her last moult; in one case the male found the female in the cage by touch alone (I could see no evidence of recognition by sight), and rendered her passive by gentle tapping with his first legs. The male of *Chiracanthium inclusum* Htz. also finds the female by touch, and tries to subjugate her

by gentle tapping with his exceedingly long first leg pair. In *Prosthesima atra* (Htz.) the male appropriates immature females; there is no courtship, beyond a tapping with the legs. The two sexes of *Geotrecha crocata* (Keys.) do not recognize each other by sight; a mating was observed, but no courtship preceded it. Also individuals of *Geotrecha pinnata* Emert, do not recognize one another by sight, perceiving each other only when nearly in contact—so probably by air pressure (touch); they run rapidly, and in the open on overcast afternoons, so they might be expected to form visual images, but they showed no signs of recognizing each other by sight. After a mating the male always leaves the female for a few moments, then returns to her again, and at such times he moves in an irregular path, feeling for her. He recognizes her by contact, and taps her legs with his own until she becomes quiet. In one case a female accepted in succession and repeatedly the embraces of two different males, one of which had only a single palpus; here the female exhibited no choice whatsoever. In the drassids, accordingly, sex recognition is wholly by touch; the male sometimes seizes the female by storm, sometimes subjugates her by tapping with his legs.

In the thomisids we have to do with diurnal spiders that lie in wait for their prey, without constructing web lines (except drop lines); they are mostly found upon vegetation above the ground. The male is more nimble than the female and smaller, sometimes much smaller. In *Micrommata virescens* Menge (1874) saw the male jump upon the back of the female. In *Xysticus stomachosus* Keys. I found (1903) that the sexes recognize each other by sight to some extent, but that the male pays no particular attention to the female until he touches her, when he quickly seizes one of her fore feet with one of his and nimbly swings around and mounts her from the rear. Last summer I saw somewhat similar behavior in *X. nervosus* Banks: the male seemed to

recognize the female only by touch, when he came in contact with her he immediately placed several of his feet upon her, she drew her legs close to her side and he got upon her dorsum. Also in *Misumena aleatoria* I saw, on several occasions, that the minute male always found the female by touch, and quickly climbed upon her. In this family, accordingly, even though diurnal, there is no courtship and the male gains the female by superior agility aided by his smaller size.

The lycosids comprise crepuscular and nocturnal species, few of them hunting in the sunlight. Menge (1877) observed a female of *Trochosa terricola* Thor. build a nest in moss, and a male lying for hours before this hole "striking lightly here and there with the palps and fore legs," after which the female allowed him to embrace her. A similar courtship was seen by Mrs. Treat (1879) in another lycosid, the species not positively identified; the female lives in a silken burrow which the male approaches very cautiously; she "slowly advances to meet him, and he slowly retreats from the mouth of the den, moving backward while she moves forward, just reaching him with the tips of her fore legs as if caressing him"; this backward and forward progression of the two is repeated many times. Observations on other lycosids have been made by me (1903). In *Lycosa bilineata* (Em.) (*L. ocreata pulchra* Montg.) the anterior male tibiæ are provided with thick brushes of vertical hairs, making them very conspicuous, but though the sexes recognize each other by sight there is no courtship; in one case a male simply jumped upon the female, in another after touching the female the male withdrew a little and quivered these legs slightly, the female moved towards him and he jumped upon her.

When the male stands before the female with these legs flexed, as he does, with the patellæ close to the sides of his cephalothorax and his body crouched near the ground, the tibiæ are more horizontal than inclined upward. This, then, is not the best attitude to exhibit them to the female. . . . On this account this bristling of the tibiæ can hardly

be regarded as a sexual ornament that is exhibited to charm the female. Further, it may be noted that this position of the first pair of legs is also assumed by the female when roughly handled or frightened; it is an attitude of defense of the species, not of sexual exhibition.

In *Lycosa ocreata* Hentz (*L. stonei* Montg.) the tibiæ of the first legs of the male are similarly provided with a brush of hairs, and he is darker than the female, but there is a pronounced courtship by the male consisting of rhythmically repeated waving of these legs with a jerking of the whole body backward and forward. Sight plays a considerable part in the approach of the sexes, but "apparently the first recognition of sex is by touch" for the courtship does not commence until the male has touched the female. In *L. lepida* (Keys.) the male is smaller and more brightly colored, and exhibits a simple courtship, shaking in the air his fore-legs that are only slightly elevated above the ground (his body may be prone). In *L. scutulata* Hentz there is "a decided courtship; the male differs from the female in his smaller size and in the black color of a portion of his fore-legs, and these legs (and the palpi also) are moved in a particular manner during the courtship. Observation shows that the male recognizes the female as such at a distance of at least six inches. The male's approach to the female is very slow, a kind of creeping, not at all similar to the vehement approach of certain other Lycosids. . . . The female, if eager, gives the signal of willingness to the male by touching him lightly with her first pair of legs, when he immediately embraces. In the observed cases, with one exception, the female killed the male at the end of the copulation." In *Pardosa nigropalpis* Emerton the males are smaller, and there "is a marked sexual color difference, the male being deep black and the female more brownish. . . . The advances are made by the male, and there is a distinct courtship process, which a vigorous male may maintain for two or three hours at a time with few interruptions when the female is recalcitrant. . . . The courtship motions are as follows:

The male stands with his body well elevated above the ground (an attitude that the female takes only when she is aggressive) on his three posterior pairs of legs, his head higher than his abdomen. . . . He waves his palpi upward in the air (*i. e.,* straightening them out before his head) and flexes them outward, from one to three times, then draws his body slightly backward and downward, rapidly waving in the air the outstretched palpi and first pair of legs, and spasmodically shaking the whole body with the violence of the movement. . . . The male recognizes a female as such immediately on touch; whether he recognizes her by sight alone I can not tell. In courting a fleeing female the male appears to follow her mainly by sight," but only when she is moving. In lycosids, accordingly, there is no courtship or else a complicated one; sex recognition is by touch alone, or by touch and sight combined.

For the attids we have a considerable number of observations. Seidel (1847) states that at the time of mating several females of *Salticus scenicus* Hahn seek out one male and live with him. But the greater part of our knowledge on this group is due to the Peckhams. In 1889 they described the courtship of *Saitis, Epiblemum, Icius, Hasarius, Synageles, Marptusa, Phidippus, Dendryphantes, Zygoballus, Habrocestrum, Philaeus* and *Astia,* with full delineations, accompanied by excellent drawings, of the posturings and movements of the male, which in some species are the most complex yet known. There are peculiar jumping and wheeling movements constituting dances, besides erection and display of those parts of the body that are strikingly colored and modified. We have not space here to repeat their descriptions and could not, indeed, do justice to them in a short summary; the reader should refer to the original memoir. In a following paper (1890) these authors describe the courtship of *Habrocestrum,* which is peculiar in that a male exhibits an ornamentation of the third pair of legs, and they give new figures of the attitudes of *Synageles picata.*

In their last study (1909) they figure the male posturings in *Pellenes* and *Euophrys*—in *E. monadnock* the male taking such a pose as to display at once the yellow palps, the heavily bristled, black first leg pairs (held elevated), and the orange femora of the third and fourth pairs.

Finally, I would add the observations made by me last summer on a considerable number of pairs of the large *Phidippus purpuratus* C. Koch. Here the male is not much smaller than his mate, but much darker and with more iridescence. A male will court on sight of a female, without first touching her; his motions consist of elevation of the cephalic region, raising and outspreading the first leg pair (with some waving of them), accompanied with advance towards the female and retreat from her as well, as side-stepping. After a male has once courted and won a mate, if he is kept in the same cage with her, he thereafter courts to much less degree, and finally not at all, before embracing her; that is, he would seem to have lost his fear of her, and indeed the pregnant female will more frequently run away from him than he from her.

IV. Interpretation of the Courtship Phenomena

Preliminaries to mating, whereby one individual seeks to gain the favor of another, constitute what we mean here as courtship. My previous definition (1903) of it, "a rhythmically repeated set of motions on the part of the male," was incomplete in limiting the process to one sex, for the female also may take part. Some mode of courtship would then occur where there is no immediate seizure of the female by the male, and where one of the individuals, generally the male, is the more eager.

In some cases there is no courtship, where the male is as large or larger than the female, the male seizing the female—sometimes appropriating her when immature, and mating with her shortly after her last moult. And in the thomisids the male captures his mate by superior agility. But more usually there is some form of courtship, and this may be by either touch or sight.

In the case of courtship by touch, the simplest form, in Drassids, *Dictyna* and some Agelenids, is that where the male after recognizing a female by touch taps her rapidly with his feet until she either runs from him or else becomes quiet and submissive to his embrace. The more complex form is found among the snare-makers, especially epeirids and theridiids, and the courtship is by signal pulls upon the lines of the snare; in this way the male not only locates the position of the female upon her snare, but he also ascertains whether she is eager for him, for when she is eager she returns his signals in the same way.

Courtship by sight is found most highly developed in the attids, less complicated in the more corpuscular lycosids, but not at all in the diurnal thomisids; it is not, accordingly, characteristic of all diurnal species. The courtship movements of the male range from a simple waving of the first leg pair, or waving of these and the palpi, to much more complicated movements of these parts associated with peculiar posturings, advances and retreats, and side-wheeling.

This brings us to the important question, just what psychical and physiological elements enter into the courtship? We can most clearly discuss this by considering the sexes in turn. Without question the chief psychical condition is sexual desire, in the case of the male, resulting from a physiological state due to internal secretions at the time of maturity. But with it is associated an inhibiting factor, the male's fear of the female. When the male is as strong as the female he exhibits no special fear of her, she is rather distinctly timorous towards him, then he does not court but seizes her by force. But in most species the male is smaller, in some cases very much smaller, than the female, and in all such instances he indicates great caution in approaching her, which is a demonstration of fear on his part. Adult females are decidedly more pugnacious than males, and contests between females are generally fatal to one of the contest-

ants, while this is seldom the result in battles between males. I once saw a male of *Phidippus purpuratus* kill his mature mate, and McCook (1890) has listed such cases in agelenids; but such happenings are very rare, and occur only when the male is as powerful as the female. In the great majority of species the male is decidedly afraid of the mature female, at least until after he has mated with her, and he does not exhibit towards her those aggressive movements which she often displays towards him. His embrace need not mollify her, for in certain epeirids and lycosids she has been seen to kill him immediately after the mating. It is interesting to note that one of the most general motions made by the male in courtship is the raising and extension of the forelegs towards the female, which is but a modification of a spider's general attitude of defense—a motion exhibited by both sexes when strongly disturbed.

Now there is courtship by the male only when he does exhibit this fear. Accordingly, we may conclude that courtship by the male spider results from a combination of the state of desire for and fear of the female. This explains satisfactorily why in some cases there is courtship while in other cases there is not. Courtship by the male continues until he has learned that the female is not hostile towards him; and his successive advance and retreat in her direction gives him the opportunity of experiencing, so learning, her degree of aggressiveness. By this courtship he advertises himself as a male, for no female shows such movements; and he may at the same time prominently display his ornamentation.

But we have neither reason to suppose that he is concious of influencing the female thereby, nor that he is conscious of exhibiting particular personal attractions towards her. For in those lycosids where there is little secondary sexual difference the male may have as complex courtship movements as in cases where he is more ornamental. And it is assuming too much about a spider's mentation to postulate that the male is not only

conscious of his beauties, which are generally so placed that he can not perceive them himself, but has also an idea that he may arouse the female's esthetic sense. The Peckhams remark (1890, p. 122):

> That whatever fine points of color or structure the male possesses, his actions before the female display them to the very best advantage; indeed, he *seems* to have a strong consciousness of every advantage, and to sedulously strive to bring it to the notice and impress its beauty upon the mind of the female to whom he is paying his addresses.

But they add a modifying foot-note:

> We do not say that, in our opinion, he *is* conscious of his strong points. It is quite conceivable that the tendency to perform the antics may have developed along with the beauties which they serve to display without any idea of this existence dawning in the mind of the spider.

I think we should, until we have evidence to the contrary, accept as the correct interpretation the suggestion stated in that last sentence of the Peckhams, *i. e.,* that the male is not conscious that he is influencing the female, either sexually or esthetically.

We have just seen that the raising of the forelegs in courtship is but a modification of the general attitude of defense, therefore not primarily for display. The waving of the palpi may follow any excitement, is also not exclusively sexual. And I have been interested in finding a partial explanation of a male attitude exhibited by certain attids, namely the lateral flexion of the abdomen. In *Phidippus purpuratus* the male at frequent intervals in his courtship touches his spinnerets to the ground, thus attaching a silken line, and then in his side-wheeling before the female, with closed spinnerets, this line pulls his abdomen to one side. Such flexion of the abdomen, at least in this particular species, is thus not a conscious effort of display, but is due to a simple tension of the ordinary drag-line.

Last summer I observed in several species the curious phenomenon of males courting one another in the same way as they do females. I placed in a small cage two males of *Pardosa pallida,* Emert., and they moved their

fore-legs and palpi just as in normal courtship. Two males, a smaller and a larger, of *Phidippus purpuratus* were placed together; the larger was aggressive, the smaller exhibited towards him normal courtship motions. In neither of these cases were females present. Two males of the brilliant *Phidippus mccookii* Peck. on being placed in one cage, on several different occasions, raised the legs, side-wheeled, and flexed the abdomen laterally, resembling a courtship; I had no female of this species to determine the normal courtship, but to females of *P. clarus* Koch they acted in the same way. Two males of *Zygoballus bettini* Peck. were placed in a cage and watched each other attentively, but without courtship; an immature female was introduced, when each male proceeded to court her: advancing and retreating by short quick steps, the first legs raised vertically and parallel and these and the palpi twitching; the female ran away from both, and after I had removed her the males exhibited the same movements towards each other, with the only difference that the raised legs were somewhat divergent and that the smaller male fled when the larger came too near. Evidently in these cases the males mistook one another for females, which would indicate that their visual discrimination is far from precise—perhaps less than that of the females.

The female is also actuated by sexual desire, sometimes quite as strongly as the male; this is the case with *Theridium tepidariorum,* where the female seems insatiable, and the Peckhams (1889) observed a female of *Saitis pulex* approaching a male with courting movements. In theridiids and epeirids the female signals along the snare lines quite as vigorously as the male does.

But whenever she is larger than the male, she exhibits as a rule, no great fear of him. She may express her desire to the male by remaining quiet and passive, or by touching him gently or moving towards him, or by counter-signalling by means of lines of her snare; and sometimes she may take the initiative in this. Where

the courtship is by touch it is wholly excluded that the female can have any esthetic appreciation of the male, she is actuated either by sexual desire or by its absence. But where there is courtship by sight, as in the attids and some lycosids, the Peckhams have consistently maintained that the female is influenced by an esthetic appreciation of the colors, ornaments and postures of the male, and that she chooses that male that pleases her esthetic sense; and they have reiterated this view in their last paper (1909) without referring to my quite different interpretation (1903). Now there is no doubt that, as the Peckhams have shown, when there is courtship by sight the female attentively watches the male, and also no doubt that the male's motions are generally such as to exhibit his colors and ornaments to the best advantage; an exception to this is that the male of *Lycosa bilineata* exhibits no courtship although his fore-legs are provided with thick brushes of hairs. Then there is the interesting observation (Peckham, 1889) that of the two male forms of *Astia vittata* the female always selects the more ornamented *niger*-variety, which is darker and possesses plumose tufts on the cephalothorax, though it is not stated in how many cases this was observed. But it is significant that the *niger*-form "is much the more lively form of the two"; it might then be the case that the female selects him not because he is more ornamented, but because he is more lively,—therefore because he more quickly advertises himself as a male. This seems to me to be the correct understanding of the matter. For just as we have no evidence that the male consciously endeavors to exhibit his attractions, we have also no evidence that the female is influenced esthetically. What we do know is that the male by his courtship, a set of motions resulting from the conflicting states of sexual desire and fear, exhibits or advertises himself as a male; and that the female on sight of this courtship recognizes him as a male and accepts him if she be eager, or else becomes gradually stimulated by watching him. In other words, too great an assumption is made in supposing the female to have an esthetic sense, while it is

more probable that the female is attracted by maleness alone and not by beauty. The immediate effect of courtship on the female is the stimulation of her sexual desire by recognition of a male, not first the arousing of an esthetic sense and second the arousing of sexual feelings. On my interpretation the case in *Astia* may be explained simply and without assumption: the *niger*-form is selected by the female because he is more different in color and structure from her than is the other male form, consequently is more quickly recognized as a male; this is what determines her, and not color or ornaments. The significance of peculiarities in color, structure and movements of the male lies in insuring quick sex-recognition, not in arousing esthetic feelings. The case of *Astia* is the only one known in which the female seems to exert a choice between males, dimorphism of males being rare in spiders.

We conclude, accordingly, that the male in visual courtship is not actuated by a conscious effort to exhibit his peculiar beauties, and that the female does not select males by an esthetic sense. Courtship by the male results simply because fear is mingled with his desire; and probably the female will accept the first male who courts her, and makes himself recognized as a male, at the time when she is physiologically desirous. Sexual selection in the meaning of Darwin, accordingly, and in opposition to the views of the Peckhams, has probably played no part in the evolution of the secondary sexual differences of spiders.

V. The Nature and Use of the Secondary Sexual Differences

We are not concerned here with the question of the first origin of secondary sexual differences, *i. e.*, whether they have arisen as gradual fluctuations, as Darwin held for the most part, or from acquired traits becoming inherited on the view of Cunningham (1900), or from sudden mutations as Morgan (1903) has suggested, for there is no observational evidence from which we can reason. We

have rather to consider their value to the species, what is their use, and the factors which have served to perpetuate them.

In the first place it will be convenient to name categorically the main classes of secondary sexual characters found in animals in general, and second to discuss the significance of those found in araneads.

Plate (1908) has furnished a useful tabulation of secondary sexual characters, devised mainly to show "what an enormous morphological field is comprised in this term." The following arrangement of such characters is simpler than his, though as comprehensive, and will be more convenient for our present discussion.

1. Weapons employed by males in their combats with one another.

2. Characters used for sexual recognition and sexual stimulation.

3. Characters of more immediate value in ensuring approximation of the sexes and copulation.

4. Characters of value in provision for and nurture of the progeny.

5. Characters due to habitudinal differences of the sexes.

6. Characters of value in protecting a particular sex against other species.

We may next determine how these six main groups of characters are represented in spiders.

1. The weapons employed by males in their fighting for the possession of females are readily explained, as Darwin did in the first part of his theory of sexual selection, as being perpetuated by a natural selection between males.

Such direct combats between males do not appear to occur among spiders, neither do the males possess peculiar weapons, unless peculiarly modified chelicera may be considered as weapons. Mature males when placed together will frequently fight, but the Peckhams (1889) in describing this in the case of an *Icius* conclude that the battles are probably sham affairs, rarely resulting disastrously, and "gotten up for the purpose of displaying

before the females." There may be more fighting between males when females are present, though there is no good evidence on this matter; but we have no reason to suppose that the males consciously endeavor to exhibit their prowess any more than they consciously try to display their ornaments. Males of the attid *Icius palmarum* placed in small cages were not observed by me to fight, nor were males of *Phidippus mccookii* nor those of *Zygoballus bettini;* but fighting of males was noticed in *Ceratinopsis interpres, Geotrecha pinnata* and *Xysticus nervosus,* and in the last species a male would leave the back of a female to fight a rival. But, as the Peckhams found, no injuries result to the males from this fighting, therefore these are not serious combats, and only once have I seen a male kill another male of his species (in *Prosthesima atra*).

Male spiders also do not possess intimidation organs, which result when weapons become hypertrophied according to the views of Guenther (1909).

2. Characters for sexual recognition and stimulation may be subdivided according to the sense organs which they affect, as Jäger (1874) has indicated. As we have seen, the only senses concerned in the case of spiders seem to be those of touch and sight. It may be that certain of the tactile organs, probably certain of the jointed spines, may become more numerous or more specialized at maturity to aid in sexual recognition, but this point remains to be tested. But those secondary sexual characters of male spiders found only where there is courtship by sight, such as ornamental colors and structures, and posturings that display them, fall under this category. Such are the male differences, as the Peckhams have shown, in the chelicera, clypeus, palpi and legs, that is, "in those parts of the animal that are plainly in view when the male is paying court to the female." Thus the chelicera may be lengthened, curved or spinous; the clypeus may be heightened or bear prominences; the first pair of legs may be thickened or have tufts of hairs; and any or all of these parts may have conspicuous color

markings. These differences are so pronounced in the attids that in many cases the species is known only by one of the sexes. We have previously seen that conscious esthetic choice by the female probably does not account for such male characters, that they are, accordingly, probably not due to sexual selection. These characters of the males may be most readily explained as being conserved by simple natural selection. Peculiar male ornamentation would be selected because it insures quicker sex-recognition, therefore prompter mating. The male is thereby more surely accepted by the female, not selected by her in the sense of Darwin. The process is much more an announcement of sex by the male than a choice by the female, and results in the female accepting the sex rather than the individual. There is no reason to suppose the female spider is actuated esthetically, while we do know she is actuated sexually—in some cases quite as much as the male is. There is no need of calling in any other factor than natural selection.

Whether such ornamental male characters, which stimulate the visual sense of the female, subserve sex recognition rather than sex stimulation we are hardly able to decide in the absence of any thorough analysis of the psychical states of spiders. But probably a female would first have to recognize a male as a male before she became sexually aroused by him, and for this reason male ornamentation would seem to be primarily to insure sex-recognition rather than sex-stimulation.

In opposition to the views of Wallace (1889) the Peckhams (1890) have correctly argued that the ornamentation of male spiders is not due to any "higher degree of vitality" of the males, for the female seems to be in all respects fully as active as the male, and at maturity even more active. The same objection may be made against the concept of Geddes and Thomson (1897) with regard to differences of the sexes.

The Peckhams have found in the attids, in agreement with Darwin's conclusions for birds, that: (*a*) when the adult male is more conspicuous than the adult female, the

young of both sexes more closely resemble the adult female; (*b*) when the adult female is more conspicuous than the adult male, the young of both sexes more closely resemble the adult male; and (*c*) when both adults are alike the young of both sexes resemble them.

3. Characters of value after the act of sex-recognition to insure efficient mating would seem to be various, and all probably perpetuated by natural selection. Such would be the relatively larger legs in the case of certain males, in so far as they serve to hold the female. Smaller size and greater agility of the male, not a frequent phenomenon in araneads, would also aid in the mating by enabling the male to move more quickly upon the snare of the female, and to escape more rapidly from her should she be aggressive.

4. Characters of value in providing for and nurturing the young are limited in spiders to the female, and are also perpetuated by natural selection. Such are the greater size of the abdomen to accommodate the eggs, and the gland whose function is to agglutinate them. Such is also the greater pugnacity and bravery of the female, which is probably the expression of her greater need of food.

5. Characters due to habitudinal sex differences are few in spiders. Such differences first become marked at maturity, as do the other secondary sexual characters, all being in some way connected with internal secretions formed during the elaboration of the genital products. In the epeirids, as shown by me (1908*b*), immature males have the same mode of life as their sisters, and "construct perfect snares of the types of those of their respective females. But the adult males . . . do not spin snares at all, but build nests near those of adult females." Indeed, it is quite general among snare-making species that adult eager males regularly leave their snares to live upon or near those of females; and adult males of lycosids and drassids, which make no snares, leave their nests to seek for females. The chief habitudinal difference, accordingly, is that while the female continues a more or less

sedentary existence, or makes excursions mainly for food, the male in many species becomes a wanderer when he is adult, searching for a mate. The male thus comes to spin less and to run more; and a morphological consequence of this habit is seen in the total or nearly complete loss of the cribellum, or spinning plate, by adult males of certain species. It is possible his relatively greater leg length may be in some cases also associated with this habit. These few cases would fall in line with the theory of Cunningham (1900), being characters due to difference in mode of life of the sexes. But all such phenomena would be likewise regulated by natural selection.

6. The secondary sexual characters which operate to protect a particular sex in the struggle for existence are found mostly in the females, and these would be subservient to natural selection. In certain spiders, as notably some epeirids (*Argiope, Acrosoma, Gasteracantha*), the female is not only much larger than the male, but also much more brightly colored, often with most conspicuous black and yellow or red and yellow markings; and in *Acrosoma* and *Gasteracantha* the abdomen may be drawn out into angular processes and spines. We can not accept for this case the Peckhams' suggestion that this is due to difference of mode of life of the sexes, for so far as is known the males of these have the same mode of life as those of other epeirids. These brilliant and remarkable females all build their snares in the open sunshine, and remain upon the centers of the snares. They would seem rather to represent cases of warning coloration, this ultimately protective to the possessors: in *Acrosoma* and *Gasteracantha* the bright markings would serve to advertise the hard and spinous abdomina, and in *Argiope*, which is soft-bellied, perhaps to announce the large snare. Possibly the brushes of hairs on the legs of another female epeirid, *Nephila*, would be an example of warning characters calling attention to the unusually large and powerful web, thus protecting the snare against birds. It is always difficult to be sure of a correct interpretation of phenomena of this kind, but it would seem probable

that some cases of brighter female coloration represent examples of warning coloration, and consequently come under our sixth category of secondary sexual phenomena.

It will be recalled that Wallace (1889) explained the general less conspicuous coloration of female birds on the ground of their need of greater protection, since they play the major rôle in guarding the nest and the eggs. The Peckhams (1889) have argued that this idea would not apply to the general inconspicuous coloration of female attids, because when they have eggs they hide these and themselves within thick silken nests and so are sufficiently protected from enemies. But I believe Wallace's explanation will apply in the case of spiders. For the males do not develop their ornamentation until maturity, and they have much less need of protection than the females because they live usually not much longer than a few weeks after maturing, and take no part in the care of the young. The males have fulfilled their main function after impregnating the females, and they are of no use to the species thereafter. But the females live at least several months after maturing, in some cases several years, and they have the whole care of the eggs and young. In araneads, as in all animals, the females are of the greater importance in the perpetuation of the race. Therefore it is probable, in agreement with Wallace, that natural selection has generally maintained a more protective coloration of the female.

In all six categories of secondary sexual characters in so far as spiders are concerned, accordingly, natural selection alone is sufficient to explain the regulation of the phenomena. At the same time these phenomena would seem to have a manifold origin, as they certainly fulfill very different uses.

LITERATURE LIST.

1873. Canestrini. Caratteri sessuali secondarii degli Arachnidi. Atti Soc. Veneto-Trentina Sc. Nat. Padova. 1.

1900. Cunningham, J. T. Sexual Dimorphism in the Animal Kingdom. London.

1886. Darwin, C. The Descent of Man, and Selection in Relation to Sex. New ed., New York.

1878. Emerton, J. H. The Structure and Habits of Spiders. Salem.
1897. Geddes, P., and Thomson, J. A. The Evolution of Sex. London.
1909. Guenther, K. Der Kampf um das Weib in Tier- und Menschenentwicklung. Stuttgart.
1874. Jäger, G. In Sachen Darwins. Stuttgart.
1887. Lendl. Ueber die Begattung und die Copulationsorgane von Trochosa infernalis Motsch. Term. Füzetek. Budapest, 11.
1890. McCook, H. C. American Spiders and their Spinning work, Vol. II. Philadelphia.
1843. Menge, A. Ueber die Lebensweise der Arachniden. *Neueste Schr. naturf. Ges. Danzig*, 4.
1866. Menge, A. Preussische Spinnen. *I. Schr. naturf. Ges. Danzig* (N. F.), 1.
1872. Menge, A. *Idem*, 5. Ibid., 3.
1873. Menge, A. *Idem*, 6. Ibid.
1874. Menge, A. *Idem*, 7. Ibid.
1877. Menge, A. *Idem*, 10. Ibid., 4.
1903. Montgomery, T. H. Studies on the Habits of Spiders, particularly those of the Mating Period. *Proc. Acad. Nat. Sci. Philadelphia.*
1908*a*. Montgomery, T. H. The Sex Ratio and Cocooning Habits of an Aranead, and the Genesis of Sex Ratios. *Jour. Exper. Zool.* 5.
1908*b*. Montgomery, T. H. Further Studies on the Activities of Araneads. AMER. NAT., 42.
1903. Morgan T. H. Evolution and Adaptation. New York.
1889. Peckham, G. W. and E. G. Observations on Sexual Selection in Spiders of the Family Attidae. Occas. Papers Nat. Hist. Soc. Wisconsin, 1.
1890. Peckham, G. W. and E. G. Additional Observations on Sexual Selection in Spiders of the Family Attidæ. *Ibid.*, 2.
1909. Peckham, G. W. and E. G. Revision of the Attidæ of North America. *Trans. Wisconsin Acad.*, 16.
1907. Petrunkevitch, A. Studies in Adaptation. 1. The Sense of Sight in Spiders. *Journ. Exper. Zool.*, 5.
1908. Plate, L. Selectionsprinzip und Probleme der Artbildung. 3te Ausg., Leipzig.
1905. Pritchett, A. H. Observations on Hearing and Smell in Spiders. AMER. NAT., 38.
1847. Seidel. Einige Beobachtungen an Spinnen. Uebersicht Arbeit. Veränd. Schles. Ges. väterl. Kultur, Breslau.
1866. Termeyer, R. M. de. Researches and Experiments upon Silk from Spiders, and upon their Reproduction. *Transl. by B. G. Wilder. Proc. Essex Inst.*, 5.
1879. Treat, M. The Habits of a Tarantula. AMER. NAT., 13.
1888. Wagner, W. Des poiles nommés auditifs chez les Araignées. *Bull. Soc. Nat. Moscow.*
1837. Walckenaer. Histoire Naturelle des Insectes, Aptères. Suites à Buffon, 2. Paris.
1889. Wallace, A. R. Darwinism. London.
1861. Westring, N. Araneæ Suecicæ descriptæ. Gothoburgi.

NOTES AND LITERATURE

NOTES ON ICHTHYOLOGY

One of the most important publications in ichthyology, as a result of the stimulus given to zoology by our knowledge of evolution, is the "Cave Vertebrates of America," by Dr. Carl H. Eigenmann, of the University of Indiana, published by the Carnegie Institution of Washington. In this large and finely printed volume is a complete discussion of caves and of cave fauna, the greater part of this fauna being composed of blind fishes. The caves of Indiana, Kentucky, Missouri and Cuba have been especially investigated in this paper. Dr. Eigenmann shows very conclusively that those species of fishes which are the ancestors of the blind forms were of those types which avoid the light, skulking in darkness in preference. Those fishes which take their prey by sight alone are never represented among the ancestors of cave fishes. Dr. Eigenmann adopts the judgment, independently reached by Mr. Garman, that the originals of the cave species (non-aquatic, especially) of Kentucky were probably already adjusted to a life in the earth before the caves were formed. Dr. Eigenmann concludes:

1. That the cave fauna is in large part the result of this formation of the caves themselves, that environment and habitat developed *pari passu.*

2. That to this original fauna have been added and are being added species (such as *Spelerpes maculicauda*) which, because they are negatively heliotropic or positively stereotropic, are gradually becoming adapted to the deeper and deeper recesses of caves.

3. That to the fauna of the larger caves may also have been added animals which had become adjusted to cave existence in crevices, under banks or rocks, etc., that is, in small caves.

4. That accident has played little or no part in developing the cave fauna.

As to the general cause of degeneration, Dr. Eigenmann is inclined to take the Lamarckian view, involving the inheritance of results of disuse. A number of species of blind lizards are discussed, as well as certain blind fishes which are not found in caves. Among these are the parasitic hag fishes, *Polistotrema stouti,* of the coast of California, and the blind goby of Point Loma, in California, *Typhlogobius,* which lives in the darkness

on the under side of shelving rocks. There is also a blind catfish, *Ameiurus nigrilabris*, which is found in caves of Lancaster County, Pennsylvania.

The best known of the blind fishes belong to the family of Amblyopsidæ. The genus, *Chologaster*, contains one species with eyes, inhabiting the channels about the Dismal Swamp and the rice canals of the South. The other species are known only from caves and cave springs, and all the representatives of the genera, *Amblyopsis*, *Troglichthys* and *Typhlichthys*, are blind.

Dr. Eigenmann has also made an elaborate study of the blind fishes of Cuba, *Lucifuga subterranea* and *Stygicola dentatus*. These two fishes belong to the family of Brotulidæ, a relative of the blennies. The origin of these forms is from marine fishes living in clefts of coral rock. This coral rock, when raised above the sea, seems to have carried these forms with it. The nearest related genus, *Brosmophycis*, still lives among the corals.

The details of anatomy, as well as the details of environment, of each of these species, are very fully illustrated in this paper, which is by far the most important one yet devoted to this subject.

Dr. Eigenmann observes in conclusion that it is absolutely certain that the caves were not peopled by a catastrophe, and, further:

> That the cessation of use was gradual and the cessation of selection must also have been a gradual process. There must have been ever widening bounds within which the variation of the eye would not subject the possessor to elimination.
>
> *Chologaster* is in a stage of panmixia as far as the eye is concerned. It is true the eye is still functional, but that the fish can do without its use is evident by its general habit and by the fact that it sometimes lives in caves.
>
> The present conditions have apparently existed for many generations, as long as the present habits have existed, and yet the eye still maintains a higher degree of structure than reversed selection, if operative, would lead us to expect, and a lower degree than the birth mean of fishes depending on their eyes—the condition that the state of panmixia alone would lead us to expect. There is a staying quality about the eye with the degeneration, and this can only be explained by the degree of use to which the eye is subjected.
>
> The results in *Chologaster* are due to panmixia and the limited degree of use to which the eye is put. *Chologaster agassizii* shows the rapid diminution of the eye with total disuse.
>
> The difference in the conditions between *Chologaster* and *Amblyopsis*, *Typhlichthys* and *Troglichthys* is that in the former the eyes

are still in use, except when living in caves; in the latter they have not been in a position to be used for hundreds of generations. The transition between conditions of possible use and absolute disuse may have been rapid with each individual after permanently entering a cave. Panmixia, as regards the minute eye, continued. Reversed selection was inoperative, for economy can not have affected the eye for reasons already stated. Simply the loss of the force of heredity, unless this was caused by disuse or the process of germinal selection, can not have brought about the conditions, because some parts have been affected more than others.

Considering the parts most affected and the parts least affected, the degree of use is the only cause capable of explaining the conditions. Those parts most active during use are the ones reduced most, viz., the muscles, the retina, optic nerve and dioptric appliances, the lens and vitreous parts. Those organs occupying a more passive position, the scleral cartilages, have been much less affected and the bony orbit least. The lens is one of the latest organs affected, and not at all during use, possibly because during use it would continually be in use. It disappears most rapidly after the beginning of absolute disuse both ontogenetically and phylogenetically. All indications point to use and disuse as the effective agent in molding the eye. The process does not, however, give results with mathematical precision. In *Typhlichthys subterraneus* the pigmented layer is affected differently from that of *Amblyopsis*. The variable development of the eye muscles in different species would offer another objection if we did not know of the variable condition of these structures in different individuals. Chilton has objected to the application of the Lamarckian factor to explain degeneration on account of the variable effects of degeneration in various invertebrates. But such differences in the reaction are still less explicable by any of the other theories.

In the *Biological Bulletin* for January, 1905, Professor Eigenmann describes two species of blind fishes, *Typhlichthys osborni,* from Horse Cave, Kentucky, and *Typhlichthys wyandotte,* from Corydon, Indiana, near the large Wyandotte Cave.

In the Field Columbian Museum, February, 1909, Dr. Seth E. Meek describes a number of species from Tropical America. These are: *Rhamdia nasuta,* from Buenos Aires de Terraba, Costa Rica; *Astyanax regani,* from Las Cañas, Costa Rica; *Cyprinodon dearborni,* from Willemstad, Curaçao, Dutch West Indies; *Girardinus vandepolli,* from Curaçao; *Pœcilia caudata,* from Turrubares, Costa Rica; *Cichlasoma punctatum,* from Buenos Aires de Terraba, Costa Rica; and *Cichlasoma frontale,* from Turrubares, Costa Rica.

In the *Proceedings of the Zoological Society of London,* 1909,

Mr. Regan describes a collection of fishes from Christmas Island, six species being new.

In the same *Proceedings,* 1909, Mr. Regan gives an interesting account of the changes in coloration in living fishes, as observed in the New York Aquarium. The same subject has been since much more fully treated by Charles H. Townsend, director of the aquarium.

In the *Annals and Magazine of Natural History,* 1909, Mr. Regan gives the name *Orectolobus ogilbyi* to the species from Torres Straits which Ogilby and McCulloch had identified with *Orectolobus dasypogon.* Mr. Regan admits that *Eucrossorhinus,* established by him for *dasypogon,* is not distinct from *Orectolobus.*

In a publication of the Station at Boulogne-sur-mer, Dr. H. E. Sauvage gives a comparative study of the peritoneum of flounders.

In the *Bulletin de la Société Belge de Géologie,* 1908, Mr. Maurice Leriche discusses new species of fishes from the Oligocene of Belgium.

In the *Annales de la Société Géologique du Nord,* Volume 37, 1908, at Lille, Mr. Maurice Leriche discusses the fossil sharks of California, with special reference to the papers of Agassiz and Jordan. Leriche regards these American species as identical with the nearest corresponding species of Europe. It is a matter in which certainty is not often possible. Most of the living species now found on the Pacific Coast are different from the corresponding living sharks of Europe, but this is apparently not true in every case, and in most cases the teeth do not show adequate differences. It is also not possible in every case to know whether different teeth represent really different species, or whether they may be from different parts of the mouth of the same animal. *Heptranchias andersoni,* from California, is identified with *Notidanus primigenius,* of Agassiz. It may be here noted that *Heptranchias* of Rafinesque is much the older of the two generic names concerned. *Lamna clavata,* of Agassiz, is identified with the European *Lamna cuspidata,* of Agassiz. Mr. Leriche states that in another paper he will show that this species belongs to *Odontaspis,* and not to *Lamna.* Mr. Leriche regards *Isurus planus, I. tumulus* of Agassiz, and *I. smithii* of Jordan, as identical. *Planus,* according to Leriche, represents the upper teeth, and *tumulus* the lower teeth, while *smithii* is the name of the front teeth of young specimens. It is not un-

likely that this view of the case is the correct one. We may notice, however, that Rafinesque's name *Isurus* is much older than *Oxyrhina*. *Carcharodon rectus*, of Agassiz, and *C. branneri*, of Jordan, are regarded as identical with *C. megalodon*, the giant shark of the Miocene fossil beds about the Atlantic. *C. arnoldi*, of Jordan, and *C. riversi*, of Jordan, Leriche regards as identical with the living *C. rondeleti*, for which the older name is *C. carcharias*. This may possibly be true of the *riversi*, but the *arnoldi* is based on a specimen far larger than that of the largest living man-eating shark, *C. carcharias*. *Hemipristis heteropleurus*, of Agassiz, is regarded as identical with the European *Hemipristis serra*. This identity was also indicated by Dr. Jordan. *Galeocerdo productus*, Agassiz, is regarded as identical with *G. aduncus*, Agassiz. Among the unidentified teeth photographed in Jordan's paper, Leriche recognizes *Aprionodon*, *Galeus* and *Squatina*.

Leriche also notes that these observations show the great geographical extension of the species of sharks, and the importance that these have in the establishment of synchronisms at great distances. It is, however, true that specific differences of many kinds exist in species of shark, without showing themselves in the teeth. It is, however, safe to recognize no species as now known by the teeth alone, unless the teeth show tangible differences.

In the same annals Leriche describes numerous sharks and other fishes from the early Tertiary about Rheims.

In the same annals Leriche describes the teeth of various carboniferous fishes of the north of France and of Belgium.

In *Science*, May 28, 1909, Mr. Barton A. Bean shows that the name of the American eel should be *Anguilla rostrata* (Le Sueur), this name being earlier than that of *chrisypa* given by Rafinesque at about the same time.

In the *Bulletin of the American Museum of Natural History*, Volume 26, 1909, Dr. L. Hussakof describes a new species of the extraordinary genus of goblin sharks, of which the living form is known as *Mitsukurina*, and the extinct species by the earlier name of *Scapanorhynchus*. Dr. Hussakof gives to this new species the name of *Scapanorhynchus jordani*. In this species, the long blade of the snout is longer than in *S. owstoni;* the eye is further forward, the gills are smaller, and there are other differences of importance. Dr. Hussakof has no doubt that *Mitsukurina* is identical with the Cretaceous *Scapano-*

rhynchus, as indicated by Dr. Woodward. The differences between these living and fossil species are no greater than the differences among the species of the genus itself. Dr. Hussakof agrees with Jordan that *Scapanorhynchus* is closely related to the Odontaspid sharks, but whether it should be placed in that family or constitute a distinct family must depend on further studies of its anatomy.

One of the most remarkable features in geographical distribution is the extraordinary number of singular animals, especially sharks and chimæras, which have been discovered in the waters of Sagami Bay, the first bay to the southward of Tokyo in Japan.

In the Mark Anniversary Volume Dr. Jacob Reighard describes in great detail the natural history of the bowfin, *Amia calva,* with colored plates of different stages in the development of this singular fish.

In the *Proceedings of the Biological Society of Washington,* 1909, Dr. Barton Warren Evermann and Edmund Lee Goldsborough describe a number of fishes from the canal zone of Panama. Of these, *Cheirodon gorgonæ,* from Gorgona, is new.

In the same *Proceedings,* 1909, Dr. Barton Warren Evermann and John Treadwell Nicholas describe the fishes of Crab Creek, in the state of Washington, and with a new species or variation of trout known as *Salmo eremogenes.* This is a very robust form, with the spots gathered on the posterior part of the body. It is apparently a variant of the cut-throat trout, *Salmo clarki.*

In the same *Proceedings,* 1909, Dr. Evermann and Lewis Radcliffe have an interesting note on *Orestias agassizii,* a singular fish from Lake Titicaca.

In the *Memoirs of the Carnegie Museum,* Volume 4, 1909, Dr. Jordan and Robert Earl Richardson have a catalogue of the fishes of the island of Formosa, or Taiwan, based on the collections of Dr. Hans Sauter. The fauna of this island is essentially tropical, and intermediate between that of Japan and that of India. Most of the two hundred and eighty-six species known are from sandy shores and bays, scarcely any collections having been made among the coral reefs. In this paper nine new species are described and elegantly figured.

In the *Annals of the Carnegie Museum,* Volume 6, 1909, are three reports of the expedition to British Guiana of the Indiana University and the Carnegie Museum. This is part of the work undertaken by Dr. Carl H. Eigenmann under the joint patronage of the two institutions mentioned, and having in view as its

final purpose a complete investigation of the geographical distribution of fresh water fishes in South America, and the relation of the barriers separating river basins to the development of new species. The first of these reports by Dr. Eigenmann gives new genera and species of fishes in British Guiana. A remarkable feature of this investigation is the discovery that almost all types of fishes found in different species in North America are represented in South America by analogies belonging to the family of Characidæ. Thus, minnows, chubs, suckers, darters and perches all have their representatives in a family which is not represented by any of these, but which practically monopolizes the waters of South America.

The second of these reports, by Marion Lee Durbin, describes one new genus and twelve new species of characins.

In the third report, Mr. Christian B. Blosser describes fishes obtained, most of them incidentally, in the West Indies and on the coast of Guiana. The following are new species: *Apogonichthys melampodus,* from St. Croix; *Bodianus stellatus,* from St. Croix; *Holocanthus lunatus,* of St. Croix; *Spheroides asterias,* St. Croix. *Chromis marginatus,* a species not previously found so far north, was also found at St. Croix.

In the *Proceedings of the Royal Society of New South Wales,* 1908, Ogilby and McCulloch offer a revision of the Australian Oretolobidæ, group of carpet-sharks, some of them known locally as wobbegongs.

In the *Records of the Australian Museum,* Allan R. McCulloch publishes studies in Australian fishes, No. 2, with descriptions of a number of new or rare species from about Sydney.

In the *Ann. Mus. Zool.* of St. Petersburg, Volume 14, 1909, Dr. Leo S. Berg demonstrates the distinctness of the genus *Acanthogobio* from *Hemibarbus.*

In the same publication, Dr. Berg discusses the trout of the Sea of Aral, *Salmo trutta aralensis.*

In the same bulletin, Dr. Berg discusses the salmon of the Black Sea, *Salmo salar labrax.*

In the same bulletin Dr. Berg gives a list of the fishes of the River Ob, or Obi, forty-two in number.

In a publication of the Provincial Museum of Natural History and Ethnology, at Victoria, British Columbia, 1909, Mr. Francis Kermode, curator, gives a list of the animals represented in the collection, with excellent photographs of many of the species of fish, as well as of birds, totem poles and other objects of

interest. This collection has the only two known specimens of the prow fish, *Zaprora silenus.*

In the *Proceedings of the Biological Society of Washington,* Volume 22, 1909, Mr. T. D. A. Cockerell, of the University of Colorado, with Mr. Otis Callaway, has a very interesting discussion of the scales of fishes, as showing their genetic relationship. In this paper the herbivorous cyprinoid fishes are treated, and it is shown that the subfamily Chondrostominæ, as recognized in America, is even more heterogeneous than was hitherto supposed. *Chrosomus* stands entirely apart from the others, its scales having the primitive sculpture of the scales of the suckers. The herbivorous forms in America are divided by Mr. Cockerell into four subfamilies: Chrosominæ, including *Chrosomus;* Chondrostominæ, including *Acrocheilus* and *Orthodon;* Campostominæ, including *Campostoma;* and Pimephalinæ, including *Pimephales* and *Hybognathus.* These last are most nearly related to the ordinary minnows.

In the same *Proceedings,* 1909, Mr. Cockerell, and Miss Edith M. Allison, continue the investigations of the scales of fishes, with photographs of several of the different types. Taking up the genus called *Leuciscus,* he suggests that probably none of this species are congeneric with the European *Leuciscus leuciscus.* If this view is correct, which seems probable, the name *Richardsonius* should probably be adopted for the American forms referred to this group. *Lavinia* is not one of the Chondrostominæ, but it is closely related to the forms called *Richardsonius.* Probably none of the American species referred to the genus *Rutilus* of Rafinesque are congeneric with the *Rutilus rutilus* which is an ally of *Leuciscus.* Mr. Cockerell separates *Nocomis,* of which the type is *kentuckiensis,* from the genus *Hybopsis,* basing the genus on the character of the scales. These are supported by numerous minor characters. *Hybopsis gelidus* is made by Cockerell the type of a new subgenus, *Macrhybopsis.* Cockerell regards *Hybopsis, Notropsis* and *Cliola* as derived from Pimephalinæ.

These investigations of the character of the scales seem likely to prove very important as indicating the real relationships of these variant forms.

In the *Bulletin of the Museum of Comparative Zoology,* 1909, Mr. Henry B. Bigelow describes the cruise of the United States Fisheries Schooner "Grampus" in the Gulf Stream during July, 1908.

In the *Proceedings of the United States National Museum,* Volume 37, 1909, Professor Oliver P. Hay, discusses the nature of the fossil sharks, with tooth-like structures, known as *Edestus,* with a description of a new species and a new genus, *Toxoprion.* Dr. Hay regards these, not as teeth, but as a succession of spines or spinal structures in front of the dorsal fin, and used as weapons of offense.

In the *Proceedings of the National Museum,* Volume 36, 1909, Professor John O. Snyder describes new genera and species of fishes obtained on the voyage of the "Albatross," in 1906, on the Coast of Japan and the Riu Kiu Islands.

In the Smithsonian Miscellaneous Collections, Volume 52, 1909, Barton A. Bean and Alfred C. Weed discusses the life history of the Alaskan fresh-water sculpin, *Cottus asper.* These little fishes are extremely greedy and destroy great numbers of salmon eggs.

The British Museum has published the first volume of an elaborate catalogue of the fresh-water fishes of Africa, by Dr. George Albert Boulenger. The first volume contains the Mormyridæ, the Characinidæ, and part of the Cyprinidæ, with a number of minor families. The book is well printed, and each of the species is represented by a good plate.

In *Zoologica,* for 1909, Mr. Edward Phelps Allis, Jr., publishes a most elaborate account of the anatomy of certain mail-cheeked fishes, fifteen species being represented in his studies, and the bony structure of each of these described with a degree of fullness not hitherto shown in any papers on the osteology of fishes. This piece of work is accompanied by admirable engravings. The only suggestion which could arise by way of criticism is that not nearly all the types of the mail-cheeked fishes are represented, and that a full comparative study in which all of them would be considered might lead to results which can not flow from merely descriptive work on a part of a large and varied group.

In the Publications of the Department of Fisheries of New South Wales, for 1908, David G. Stead discusses the beaked salmon, *Gonorhynchus,* and its distribution in Australia.

In the same publications, Mr. Stead describes a number of new species of fish from the coasts of New South Wales.

In the *Annals of Queensland Museum,* Number 9, for 1908, Mr. J. Douglas Ogilby describes a number of new species and genera from the coast of Queensland.

In a second paper Mr. Ogilby discusses the toad-fishes of Queensland. A new genus, *Batrachomœus,* is proposed for *Batrachomœus minor,* and a new genus, *Coryzichthys,* for *Batrachoides diemensis.* This differs from the American genus, *Marcgravia,* in a much smaller number of fin rays. A new generic name, *Halobatrachus,* is proposed for *didactylus* of the Mediterranean. This differs from *Batrachoides* in the presence of an axillary pore.

In the index to the meeting of the British Association for the Advancement of Science, at Winnipeg, 1909, Professor W. A. Herdman gives an interesting discourse on "Our Food from the Waters," the investigation of plankton being made especially prominent.

In the *Memoirs of the American Museum of Natural History,* Volume 9, 1909, Professor Bashford Dean presents studies on fossil fishes (sharks, chimæroids and arthrodires). This contains, among other things, an elaborate study of the genus *Cladoselache,* a Devonian shark, and one of the simplest, as well as the earliest, representatives of that group. Elaborate drawings are given of the structural characters of *Cladoselache,* and the final conclusion is that these represent better than any other the primitive shark. Dr. Dean agrees with Woodward that if the earliest true fish could be found, it would almost certainly fall within the subclass to which belong our modern sharks; and the fundamental characters of the cladoselachian have given us a less ghostly picture of a direct vertebrate ancestor.

As to the arthrodires, Dr. Dean thinks that the present evidence does not lead us to affirm that these fishes possessed paired appendages homologous with pectoral and pelvic fins. There is still, therefore, a great gap remaining between these forms and the true fishes.

In the *Bulletin of the United States Geological Survey,* Mr. Earle Bernard Phelps discusses the subject of the pollution of streams by sulphite pulp waste. The exclusion of these forms of waste from the streams is one exceedingly important in the protection of the fishes of our rivers. Thus far, it has not been possible to prevent the flow of these mischievous substances into the streams, and their value for utilization in other ways is very slight. According to Mr. Phelps, the best promise seems to be along the line of the formation from sulphite of the dye called "lignone." These substances dye wool directly, giving

brilliant yellow, brown and green colors that are fast to soap, acids and alkalies, and reasonably fast to sunlight.

In the Report of the Commissioner of Fisheries of British Columbia, 1908, Mr. John Pease Babcock, Commissioner, discusses the salmon problems of Puget Sound, and the Treaty of April 11, 1908, under which the International Fisheries Commission has been organized.

In the *Comptes Rendus* of the Academy of Sciences of Paris, M. A. Cligny describes a new genus of Zeinæ, called *Parazenopsis,* from Morocco. The species is *Parazenopsis argenteus.*

In the *Annales de la Station Aquicole de Boulogne-sur-Mer,* M. Cligny shows that the species called *Harengula latulus* is nothing but a common sprat, *Clupea sprattus.* The species called *Meletta phalerica* and *M. mediterranea* are also identical with the common sprat, the Mediterranean representatives of which form a geographical race, distinguished from the races in the north of Europe, having one less vertebra, and a few less of the anterior rays.

In the same *Annales,* M. Cligny discusses the genus of *Scorpœnidæ* called *Helicolenus.* He shows that three distinct species have been confounded under the name of *maderensis.* Of these, the first, that of Cuvier, should disappear, being a simple variety of *Scorpœna scrofa;* the second, that of Lowe, should receive a new name; the third, that of Goode and Bean, should disappear, being a synonym of *Helicolenus dactylopterus.*

M. Cligny, however, conscientiously refrains from giving this new species a distinctive name, because he has never had any specimen in hand.

Nous ne nous permettons pas de donner un nom nouveau à une espèce que nous n'avons jamais eue sous les yeux; nous laissons ce soin à un naturaliste plus favorisé et qui pourrait donner en même temps un diagnose précise de l'espèce.

In the same *Annales,* M. Cligny discusses in greater detail the new genus *Parazenopsis.*

In Notes from the Leyden Museum, Volume XXXI., 1909, Professor Max Weber, of Amsterdam, describes new fishes obtained by the Siboga Expedition to New Guinea and neighboring waters. A number of new species are added to the endlessly rich fauna of the Island of the Tropical Pacific. *Rhabdamia* and *Siphamia* are new genera related to *Apogon.*

In another paper, in connection with the aquarium at Amster-

dam, Dr. Weber adds a new *Fierasfer* to the list of Siboga fishes.

In the AMERICAN NATURALIST, Vol. 42, 1908, Professor Cockerell describes the results of the expedition to Florissant in 1908. In this wonderful deposit of fossils, specimens of *Trichophanes foliarum,* of Cope, were found. This little fish, of which figures are given by Professor Cockerell, appears to belong to the suborder Xenarchi, and to be a near relative of *Aphredoderus,* a living pirate perch. It shows also relationships with the trout perch, *Percopsis,* like *Aphredoderus,* a relic of a waning fauna.

In the *Records of the Canterbury Museum,* Volume 1, 1909, Mr. Edgar R. Waite gives an account of the scientific results of the New Zealand Government Trawling Expedition of 1907. A number of new species are described, one of the most interesting being the blind torpedo, *Typhlonarke aysoni.* Only the sharks and rays are discussed in this first paper.

Under the head of "Salmon Scales as Indicative of the Life History of the Fish," Mr. J. Arthur Hutton publishes, in the form of a paper read before the Manchester Anglers Association, an article in which he shows that the scales of salmon are obtained through life, and that in a general way the age of the fish can be shown by the scales. During its stay in fresh water, the scales rapidly disintegrate, and are restored again when the fish returns to the sea. In his judgment the European salmon does not live over eight or nine years. Mr. Hutton illustrates his thesis by numerous photographs of salmon scales.

In the *Memoirs of the Indian Museum,* Volume 2, Dr. N. Annandale, the superintendent of the museum, commences a report of the fishes taken by the Bengal Fisheries Steamer "Golden Crown." The first paper treats solely of the rays, of which numerous species were obtained. One partially blind torpedo is given the name *Bengalichthys.*

In the *American Journal of Obstetrics,* Dr. Charles R. Stockard discusses the formation of cyclopean monsters among fishes by bringing up the little fishes in solutions of sea water containing chloride or nitrate of magnesium.

The same subject is discussed by Dr. Stockard in the *Anatomical Record,* Volume 3, 1909.

In the *American Journal of Anatomy,* Volume 9, 1909, Dr. Harold D. Senior discusses the development of the heart in the shad, with descriptions and bibliographies.

In the *Anatomical Record,* Volume 1, 1907, Dr. Senior dis-

cusses a number of rows of valves in the arterial bulb in the heart in different fishes. In most bony fishes the arterial bulb contains a single tier of valves. In *Albula*, there are two tiers; in *Elops* one tier; in *Pterothrissus* two tiers.

In the *Philippine Journal of Sciences*, Volume 4, 1909, Mr. Alvin Seale discusses very fully the fishery resources of the Philippine Islands, with colored plates of a number of the more valuable species.

In the same *Journal*, Mr. Seale discusses the sponge fisheries of the Philippine Islands, with numerous plates.

In the *Biological Bulletin*, Mr. H. H. Newman, of the University of Texas, discusses hermaphroditism in fishes, a bisexual specimen of *Fundulus* being considered, this being a species in which the females are very differently marked from the males.

In the same *Bulletin*, Mr. Newman discusses the contact organs, fine papillæ found on the scales and fins in certain fishes. These have been studied by him in species of *Fundulus* at Woods Hole. These contact organs are supposed to give the fish greater sensitiveness, and also, perhaps, to increase the frictional surface of the animal.

In the *Journal of the College of Science in the Imperial University of Tokyo*, 1908, Mr. Shigeho Tanaka describes six species new to science from the East Coast of Japan.

In the same *Journal*, 1909, Mr. Tanaka describes eleven new species of Japanese fishes, one of them being a new genus of Chimæras called *Anteliochimæra*. This form has a snout produced in a long spatula, as in *Rhinochimæra*. It adds one more to the dozen or so extraordinary forms of sharks, skates and chimæras which have been obtained within the last ten years in the Bay of Sagami.

This specimen was caught, probably, with a hook, in a depth of four hundred fathoms, by the remarkable collector employed at the Seaside Laboratory at Misaki, Kuma Aoki. This unlettered fisherman, who can not read even his own language, is in his way one of the cleverest ichthyologists in Japan.

In the *Annotationes Zoologicæ Japonenses*, Volume 7, 1909, Mr. Tanaka discusses a collection of fishes from the interior province of Shinano.

In the *Proceedings of the United States National Museum*, 1909, Jordan and Richardson give a review of the sea bass, or Serranidæ, inhabiting the waters of Japan, with numerous figures, and the description of one new species. When this series

of monographic reviews of Japanese fishes was begun, in 1900, as a result of the exploration made in that year by Jordan and Snyder, it was hoped that they would serve to furnish to Japanese naturalists a record of the literature largely inaccessible to them, and a record of descriptions of the known species. It was hoped at the same time that making the literature of the subject compact in this way would lead to a rapid development of the science of ichthyology among the Japanese naturalists themselves. The recent publications of Ishikawa, Kishinouye Otaki, and Tanaka, and of other students of the great teacher, Mitsukuri, have shown that these expectations have not been disappointed.

In the *Revista Universitaria,* Peru, 1909, Professor Carlos E. Porter, of Valparaiso, gives a list of the most important fishes on the coast of Chili and Peru. Forty-six species are enumerated.

In the Publications of the Bureau of Science of the Philippine Islands, at Manila, Jordan and Richardson give a check list of the fishes known to inhabit the waters of the Philippine Islands. This list, based on the various collections made under the auspices of the United States Government, and of the local Bureau of Science, now numbers eight hundred thirty species. It is probable that a full enumeration of the fishes of these islands will rise to double that number. One new genus is proposed in this paper, *Vespiculus* for *Prosopodasys gogorza.*

DAVID STARR JORDAN.

ENTOMOLOGY

A New Catalogue of Hemipterous Insects.[1]—A catalogue exhibits the taxonomy of a group in its most condensed form. For this reason, it is as interesting and valuable to a specialist as it is uninteresting and unintelligible to one who has paid no attention to the particular order or family it represents. It has indeed a very high value for the uninitiated, inasmuch as it gives him a clue to the literature on and affinities of any particular form he may need to investigate; but it is only the specialist, who has long worked on the group, who can at once appreciate its dramatic significance. The present reviewer ventures to consider himself an hemipterist of a sort, but his particular speciality has been the tail of the order, as it were, while the first volume of the new

[1] "Catalogue of the Hemiptera (Heteroptera)," by G. W. Kirkaldy, Vol. I, Cimicidæ. Berlin, F. L. Dames, 1909.

catalogue deals with the head. This circumstance furnishes a rather welcome excuse for avoiding all discussion of the numerous disputable points in nomenclature with which the work necessarily bristles, but leaves the way free for remarks of a more general character.

It is interesting to find that the new catalogue of an order so extensive and of such economic importance as the Hemiptera has been written in the Hawaiian Islands, and published in Berlin. One would have supposed it equally impossible to prepare a work of this sort in such a remote locality, and to persuade a German publisher to bring it out—in English! Here it is, however, and so far as a rather careful scrutiny reveals, it is well above the average of such things in completeness, accuracy and typographical excellence. The price also is reasonable enough. It is no mere list of names and references; localities, food-plants and natural enemies are fully cited, while great care is taken to include all sorts of biological and even embryological citations. At the beginning of each subfamily or tribe is a table showing the geographical distribution of the genera, bringing out various interesting facts. Thus the great number of genera of Acanthosomini in Australia and in the Chilean region is remarkable, and might be offered as an additional argument for a former land connection. It appears, however (if the genera are arranged in correct systematic order), that the Australian and Chilean types stand far apart and are more related to those of the Oriental and Palæarctic regions than to one another. All fossil species are included: the author remarking that "considerable misapprehension regarding these is due to the absurd and unphilosophical separation, by most geologists, of present (*Pleistocene*) time from the rest of the Tertiary (or Kainozoic) as a "Quaternary" epoch. The feeling that the species of the older Tertiary periods are the direct precursors, with but little separation in (geologic) time, of present-day forms, is thus lost, and these interesting relics are regarded with indifference by the majority of entomologists." While it is not probable that the present geological classification will be abandoned, it must certainly be admitted that if the Tertiary period is made to include an amount of time at all comparable to that of its predecessors, we are not merely in the Tertiary at this moment, but not far from the beginning of it!

T. D. A. COCKERELL.